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BRITISH MEDICAL JOURNAL

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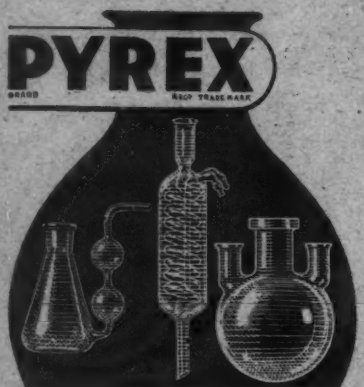

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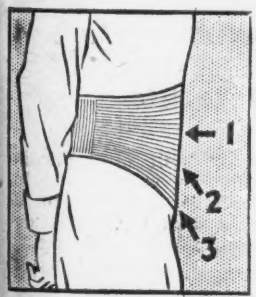

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BRITISH MEDICAL JOURNAL

LONDON SATURDAY NOVEMBER 10 1945

THE USE OF MICRO-ORGANISMS FOR THERAPEUTIC PURPOSES*

BY

Sir HOWARD W. FLOREY, M.D., Ph.D., F.R.S.

Professor of Pathology, University of Oxford

There can be no one in the whole history of surgery who more deserves commemoration than Lister, and to be asked to deliver a memorial lecture is at once a great honour and a somewhat heavy responsibility. I am not a surgeon, and it did not seem appropriate for me to lecture on an exclusively surgical subject, but as Lister in his own person combined great capacities as an experimentalist with an ability to make epoch-making innovations in surgery, I thought that to-day it might be permissible to treat of some of the scientific background which is becoming more and more important in the practice of surgery.

It may not be realized by all that Lister made some most important contributions to bacteriology—a subject which was to him of the greatest significance when he had grasped the bearing of Pasteur's observations on his own problem of "putrefaction." Lister's aim was essentially what is now called aseptic rather than antiseptic, his object being to exclude all organisms at operation, including those of the air, by means of chemical antiseptics. It is true that he used antiseptics in the treatment of established infections, but his main aim was the exclusion of pathogenic bacteria, which he at first conceived to be those floating in the air. I can imagine that Lister would be delighted to know that in the course of time some of these same organisms have been made to yield substances with almost ideal properties for the treatment and prevention of sepsis. It is a widespread idea that our knowledge of the existence of such substances is of recent origin, so I have thought it might be of interest to trace the development of this subject, partly to give a background against which to measure modern achievements and partly to help to orientate further work from both the biological and the more strictly surgical points of view.

Early Conception of Antibiosis

The antiseptic substances which we now know to be produced by many micro-organisms are beginning to be known as antibiotics. The word "antibiosis" was first used by Vuillemin in 1889. He wrote:

> "The lion that springs on its prey and the serpent that poisons the wound before devouring its victim are not considered to be parasites. There is nothing equivocal about it—one creature destroys the life of another in order to sustain its own, the first being entirely active and the second entirely passive; one is in unrestricted opposition to the life of the other. The condition is so simple that it has never been named, but instead of being examined in isolation it can be viewed as a factor in more complex phenomena. For simplicity we shall refer to it as *antibiosis*; the active participant will be the *antibiote*."

Marshall Ward blessed the word in 1899, and it has been frequently used since. The use of the word antibiotic for the actual chemical substances involved in bacterial antagonism was recently introduced by Waksman.

Just as Pasteur's conception of bacteria as the cause of disease was the starting-point of much of Lister's work so Pasteur, with his colleague Joubert, was the first to describe in 1887 the existence of this antibiosis affecting a disease-producing micro-organism. They were working on anthrax, and wrote:

> "Neutral or slightly alkaline urine is an excellent medium for the bacteria. If the urine is sterile and the culture pure the bacteria multiply so fast that in the course of a few hours their filaments fill the fluid with a downy felt. But if when the urine is inoculated with these bacteria an aerobic organism, for example one of the 'common bacteria,' is sown at the same time, the anthrax bacterium makes little or no growth and sooner or later dies out altogether. It is a remarkable thing that the same phenomenon is seen in the body even of those animals most susceptible to anthrax, leading to the astonishing result that anthrax bacteria can be introduced in profusion into an animal, which yet does not develop the disease; it is only necessary to add some 'common bacteria' at the same time to the liquid containing the suspension of anthrax bacteria. These facts perhaps justify the highest hopes for therapeutics."

Pasteur himself seems to have done no more work on this phenomenon, but in 1885 several papers of considerable interest appeared. The most important of these was by Babès, who demonstrated on solid as well as in liquid media that one organism can elaborate a substance which will stop the growth of another. He wrote that he had "studied experimentally the way in which bacteria of a known species produce chemical substances or modify the culture medium in such a way as to harm bacteria of other species. If the study of the mutual antagonisms of bacteria were sufficiently far advanced a disease caused by one bacterium could probably be treated by another." And later in his paper he said: "A further and wider study of this reciprocal action of bacteria may lead to new ideas in therapeutics." In this year, 1885, also appeared a paper by Catani, who opened with the words: "The known fact that certain bacteria can destroy other, even pathogenic, microbes if they come in contact with them in any way gave me the idea of exploring this procedure for the treatment of various infectious diseases." He took an organism which was called *Bacterium termo* (it appears to have been a mixture of species) and showed its lack of pathogenicity for animals. He then treated a patient suffering from severe tuberculosis by insufflating

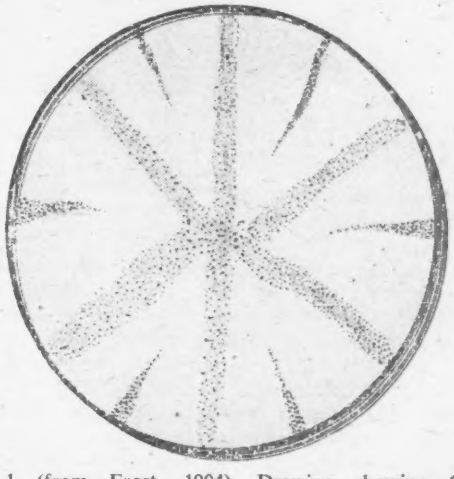

FIG. 1 (from Frost, 1904).—Drawing showing Garré's method of studying antagonism by means of alternate streaks. The large streaks of *Ps. fluorescens* were allowed to grow for 24 hours and then streaks of *Bact. typhosum* were made. These grew only where they were furthest from the streaks of *Ps. fluorescens*.

* The Lister Memorial Lecture, delivered at the Royal College of Surgeons of England, Oct. 11, 1945.

directly into the lungs by means of an atomizer a culture mixed with gelatin. The tubercle bacilli disappeared from the sputum and *Bact. termo* appeared in their place, while at the same time the patient's condition improved. He suggested that this method of bacteriotherapy might be applicable to infected surfaces. This, then, was the first example of the idea of replacing one organism by another less harmful—an idea which, we shall see, frequently recurs in succeeding years.

In 1887 an important bacteriological contribution was made by the Swiss, Garré. He introduced methods for the examination of bacterial antagonism which differ little from those in use at the present day. Fig. 1 is an illustration of one of Garré's methods which was given in a paper by Frost nearly 20 years later. In this the streaks of the antagonist and the organism to be antagonized are arranged alternately, radiating from the centre, but sometimes the streaks were put down parallel to one another at increasing distances. Garré wrote:

"I inoculated on the untouched cooled [gelatin] plate alternate parallel strokes of *B. fluorescens* and *Staph. pyogenes*. This was carried out so that the distance between the inoculated strokes increased from 3 to 15 mm. *B. fluorescens* grew more quickly. Its products of secretion diffused into the surroundings and were completely inhibitory for the near-by staphylococcal inoculation. . . . It is thus not a question of overgrowth or crowding out of one by another quicker-growing species, as in a garden where luxuriantly growing weeds kill the delicate plants. It is also not due to the utilization of the available foodstuff by the more quickly growing organism, but it is a question of antagonism caused by the secretion of a specific, easily diffusible substance which is inhibitory to the growth of one species but completely ineffective against the species producing it."

He also had the idea of using this phenomenon in therapeutics.

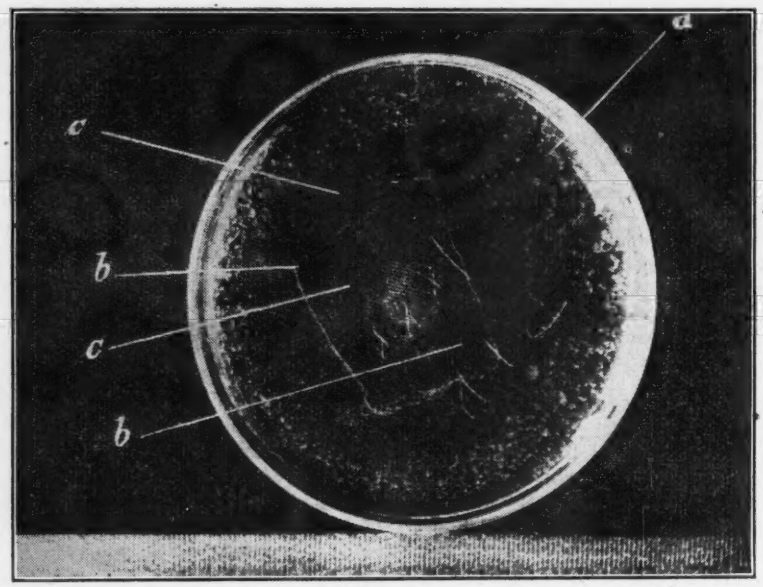

FIG. 2 (from Doehle's thesis, 1889).—A plate was poured with gelatin containing anthrax bacilli. On the surface was planted a square of *M. anthracotoxicus*. Surrounding this square is a zone in which no anthrax colonies have developed, owing to the diffusion of an inhibitory substance from the micrococcus.

There were other publications on the same subject about this time; but the work of Doehle, who published his thesis in 1889, is of outstanding interest, as it contains the earliest illustration of antibiotic action that I have encountered (Fig. 2). Doehle mixed anthrax bacilli with melted gelatin and poured the mixture into a dish and then planted in a square on the surface a certain micrococcus which he had isolated and called "*Micrococcus anthracotoxicus*." After incubation the square where the micrococcus had grown was surrounded by a clear zone in which the anthrax bacilli were completely inhibited. This technique and Garré's differ little, if at all, from those employed by some workers at the present day. Doehle hoped to use his organism for the protection of animals against infection by *B. anthracis*, but his results were indecisive.

Non-specific Immunity

While these clear-cut bacteriological observations had definitely shown that there were micro-organisms which produced substances harmful to the growth of some pathogenic bacteria another approach to bacteriotherapy was being made, which had as its basis the idea of a non-specific immunity produced by one type of organism against another. Clinicians had for long believed that an attack of erysipelas was beneficial in certain chronic infections, and in 1883 Fehleisen deliberately treated a case of lupus by injecting streptococci obtained from a patient with erysipelas. In 1887 Emmerich made observations on anthrax infection which showed that an injection of streptococci enabled a substantial proportion of rabbits to survive a subsequent dose of anthrax bacilli large enough to kill all the control animals. Moreover, the intravenous injection of streptococci into rabbits already showing "clinical" anthrax prevented the death of 60%. Emmerich demonstrated that there was no antagonism between the two organisms *in vitro* on the surface of a solid medium, so he ascribed his results to stimulation by the cocci of the body cells, which were then able to demolish the bacilli. This work was confirmed by several people, some of whom used other organisms than the streptococcus and in some cases killed the cultures before injection.

A paper by Bouchard in 1889 started work on another organism which still continues even at the present time. He published results showing that the inoculation of *Ps. pyocyanea* conferred a considerable degree of protection against *B. anthracis* infection in rabbits, though in guinea-pigs the results were not so good. This same organism was actually injected by Rumpf in 1893 for the treatment of typhoid fever in man, but the good results which he claimed were not confirmed by Kraus and Buswell (1894) or Presser (1895).

Local Application of Bacterial Products

During this period a number of authors contributed to the further accumulation of knowledge, but from a clinical point of view the observations of Honl and Bukovsky (1898) are of most interest. These Russian investigators described in enthusiastic terms the results of the local application of "proteins" from cultures of *Ps. pyocyanea*. They thought that they might accelerate the healing of ulcers by applying the proteins or products of micro-organisms isolated from the ulcers, but gave no details of how they made their preparation. They treated by local application more than a hundred infected ulcers of the leg and similar cases, some of which were so bad that amputation was under consideration, and they were very pleased with their results.

Unconnected with this work, Emmerich and Löw in 1899 introduced the first antibacterial extract into medicine. This extract, which they called pyocyanase, was capable of lysing suspensions of *B. anthracis in vitro* in a short time, and was also bactericidal to *Bact. typhosum, C. diphtheriae*, staphylococcus, and *P. pestis*. It was prepared from old cultures of *Ps. pyocyanea*. They performed therapeutic experiments on animals with the substance, but their views as to the rationale of the treatment are not by any means clear. Apparently they became confused between their knowledge of its antibiotic effect and their wish to bring it into line with the prevailing views on immunity.

Following Emmerich and Löw's work there was a remarkable outburst of energy both in the laboratory and in the clinic. Laboratory work fully confirmed that the extract was bactericidal, but controversy raged over the nature of the substance. Emmerich and Löw maintained that it was an enzyme (that is, a protein), while other workers found good evidence that it was a "lipoid" (that is, soluble in certain organic solvents) —a view which is confirmed by recent work.

A few years after the beginning of the century the idea of using it in the clinic by injection was replaced by that of using it as a local antiseptic. There can be no doubt that it was bactericidal, and Emmerich and Löw also thought that it was more harmful to bacteria than to animal cells. This important

idea of differential toxicity, which so largely governs the choice of antiseptics to-day, must have been widespread at that time, for Escherich wrote in 1906: "The resumption of these endeavours first became possible when the march of science made known to us a substance which possessed a high bactericidal capacity without at the same time harming the tissues as do previously known antiseptics. This material is the bactericidal substance from the autolysis of bacteria, discovered by Emmerich and Löw." In 1913, in a review of the position, Sonnenberger echoed these views, for he wrote: "The use of pyocyanase as a therapeutic agent is based on exact theoretical considerations. These are backed by clear-cut experiments both *in vitro* and *in vivo*. Its harmlessness within the limits of its usual employment is well established. Its capacity for healing in a large number of diseases has been proved in the clinic. Pyocyanase may be considered an important addition to therapeutics. Its widespread use is recommended within the limits given in this work." This material was very extensively used in the clinic. Many papers deal with its employment as a local antiseptic in diphtheria both for acute cases and for carriers. Meningococcal carriers were also, it was claimed, successfully treated, and the suggestion was made that it might be useful as a prophylactic spray during epidemics. It was used for conjunctivitis and other eye infections, for injection into abscesses, for the treatment of infection of the accessory nasal sinuses and diseases of the mouth such as Vincent's angina. It was injected into the trachea and even injected intrathecally in cases of meningitis. Many of these procedures have their parallel in recent uses of penicillin. It was used in veterinary medicine for streptococcal mastitis by injecting it into the udder, much as gramicidin and penicillin are used at the present day, and in addition both chemical and pharmacological examinations on a considerable scale were done to determine its properties. Thus it can be said with some truth that there is little that has been done with penicillin which was not attempted with the earlier antibiotic so far as the means then available allowed.

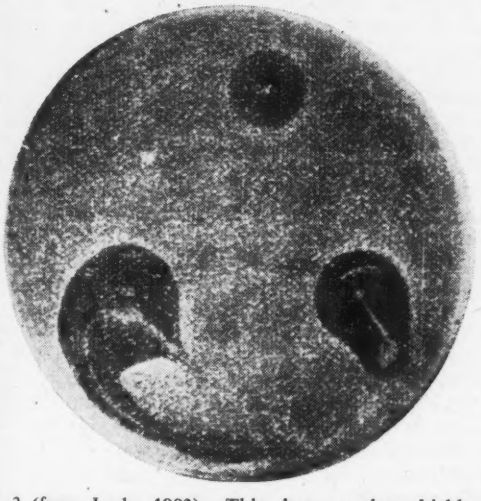

FIG. 3 (from Lode, 1903).—This shows a plate thickly sown with *M. tetragenus*. Colonies of an accidental air contaminant —a coccus—have been planted at three places, and around them are zones in which the growth of *M. tetragenus* is inhibited.

While this work was being actively pursued other investigations of great interest were recorded. In 1903 Lode wrote of an accidental contaminant he had found while preparing a plate of *M. tetragenus* for class demonstration purposes. This contaminating organism, which was a Gram-positive coccus, was subcultured and shown to produce a diffusible inhibitory substance (Fig. 3). It strongly inhibited anthrax bacilli and *Staph. aureus*, but not *Bact. coli* or Friedländer's bacillus. Lode did considerable work on the production of the antibiotic in fluid cultures, and showed that it was bactericidal. It was not an enzyme, though it was thermolabile, being inactivated slowly by heat. It could be dried by vacuum distillation and was soluble in alcohol but not in ether. Neither the micro-organism nor its metabolic products were toxic to animals, but unfortunately in artificially produced infection in mice the product had no chemotherapeutic effect. Nevertheless, this work is remarkable for the comprehensive and painstaking investigation which it displays.

In 1904 Frost published an important paper in which he described as many as seven different technical procedures for the investigation of bacterial antagonism both on solid and in liquid media. One of his most interesting techniques was to interpose a collodion membrane between culture fluid containing the organism he was trying to inhibit and culture fluid containing the organism which he suspected of producing an antibiotic. Fig. 4 is his illustration of this technique.

FIG. 4.—Frost's (1904) collodion sac for the study of bacterial antagonism. One organism is sown in the broth in the sac and another in the broth outside it in the flask. Inhibitory substances can pass through the collodion membrane, but the bacteria are kept apart.

The First Crystalline Antibiotic from a Mould

Somewhat before this time the first example of a crystalline antibiotic obtained from a mould was recorded. The mould was a *Penicillium* and the antibiotic is now called mycophenolic acid. Gosio, who did this work in 1896, showed that the crystalline material inhibited the growth of anthrax bacilli, but regretted that he was unable to perform any animal experiments owing to lack of material.

Chemotherapeutic Experiment

Nicolle demonstrated in 1907 the existence of bactericidal substances produced by *B. subtilis*; but of more immediate clinical interest are observations published by Rappin in 1912 in a journal which probably circulates little outside France. He found that *B. subtilis*, *B. mesentericus*, and *B. megatherium* acted strongly on *B. tuberculosis in vitro* and that the effect could be produced by filtered broth cultures. He went on to say:

"After having established a long time ago that these diastases [as he believed the active substances to be] were very active I naturally went on to consider whether they might not have important effects on the evolution of the tuberculous process. Nearly two years ago I started an investigation with the object of verifying this hypothesis. Several guinea-pigs which had previously been infected with tuberculosis were injected with filtered cultures of *B. mesentericus*, and I possess to this day one of the first series of animals which, treated in that way, lived more than 21 months after a virulent inoculation, with every appearance of perfect health. Another which died after 9 months had put on weight regularly and shown no sign of illness, and no lesion was seen at necropsy except a swelling at the site of inoculation and one in the opposite side. It is a very important fact that scrapings from these swellings inoculated into another animal have not so far

produced in it any sign of a reaction. When this experiment was repeated on other guinea-pigs similar results were obtained, and I have several animals which were treated in this way at varying intervals after infection with tuberculosis which are apparently in good health, while the controls have died with the classical lesions of experimental tuberculosis. These results have been obtained with a relatively small number of injections. Some animals received only two injections of filtered *mesentericus* broth in doses of one to two cubic centimetres given subcutaneously, and others the same amount into the peritoneal cavity. Neither group appeared to be disturbed by the injections, and there were no reactions."

Vaudremer wished to dispute the question of priority with Rappin, for he had found about the same time that *Aspergillus fumigatus* would also "digest" the tubercle bacillus. It is now known that this mould produces four powerful antibiotics, at least one of which—helvolic acid—has been shown to be active *in vitro* against *M. tuberculosis*. In 1913 he reported animal experiments, and went on to say that he had injected *A. fumigatus* filtrates into man, in the following terms (the italics are Vaudremer's):

"From the point of view of the treatment of tuberculosis in man any conclusion is premature. *Since 1910 we have treated more than 200 patients with extracts of A. fumigatus* in several hospitals and sanatoria in Paris. From the cases we have observed it is justifiable to conclude that the injections, which have never caused any febrile reaction, are harmless. Sometimes healing which we had not hoped for has taken place unexpectedly. In other cases there has been transient improvement, but unfortunately there are still many instances in which the tuberculous process continues to take its course."

He had obviously been trying to get a direct effect on the tubercle bacillus *in vivo* by injecting a mould metabolism solution.

Replacement Therapy

It will be recalled that Cantani had tried in 1885 to replace the tubercle bacillus with *Bact. termo*, and in 1909 this idea received another application. Schiøtz, a Danish physician, noticed that a patient with a staphylococcal infection, wrongly diagnosed as having diphtheria and placed in a diphtheria ward, did not develop the disease. Schiøtz then deliberately sprayed suspensions of staphylococci into the throats of carriers with, he claimed, good results. Many other people tried a similar procedure at different times, and some claimed that benefit was obtained—at any rate, none mentioned any disastrous infection from the staphylococcus. It may sound a somewhat absurd and empirical procedure in the absence of bacteriological information, but it is, as a matter of fact, true that many strains of staphylococcus will inhibit the growth of the diphtheria bacillus—as was shown, for example, by Dujardin-Beaumetz in 1932. In 1915 Colebrook suggested using the pneumococcus in the same way to combat the meningococcus in carriers, following on an *in vitro* observation of antagonism. Later still, in 1921, the pneumobacillus of Friedländer was suggested by Papacostas and Gaté for the treatment of diphtheria carriers.

The best-known example of replacement therapy, however, was furnished by the suggestion of Metchnikoff for the displacement of harmful gut organisms by the Bulgarian bacillus, which was later to give place to *B. acidophilus*. There is a very considerable literature on this subject, into which we will not go except perhaps to call attention to the fact that the lactic acid bacillus was used for the treatment of various infections in addition to those of the intestine. Newman in 1915, in this country, was treating cases of cystitis by injecting lactic acid bacilli into the bladder, and many authors treated infections of the nose, mouth, and sinuses, including diphtheria. Cultures of the bacilli found application in the treatment even of puerperal fever, breast abscesses, and other infections. The good clinical effects were ascribed to the production of lactic acid, which stopped the growth of the pathogens.

Another interesting replacement was suggested by Nissle in 1916. He maintained that certain strains of *Bact. coli* were antagonistic to other and more harmful organisms and that these antagonistic strains should be established in the gut: he even went so far as to say that newborn infants should be deliberately given these desirable strains. He put on the commercial market a preparation of living *Bact. coli* called "mutaflor," which apparently had a considerable sale. Here again there is pictorial evidence that such antagonistic strains of *Bact. coli* exist; for Gratia, who had described such an antagonism in 1925, figured this organism in 1932 (Fig. 5).

In connexion with the idea of "fighting" one intestinal bacterium with another, an interesting line of reasoning was displayed by Nitsch (1908). Apparently at that time two towns in France, Lyons and Versailles, were notorious for their freedom from epidemics of cholera. Nitsch reasoned that the only thing of which all the inhabitants of Versailles partook in common was air, and so it occurred to him that bacteria in

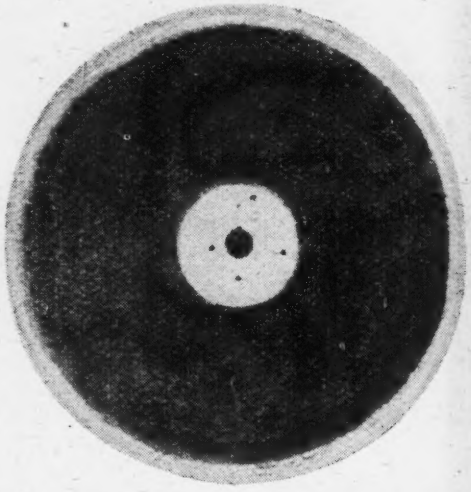

FIG. 5.—Gratia's (1925 and 1932) demonstration of the inhibition of one strain of *Bact. coli* by another. The strain sown over the plate is inhibited round the central colony of the other strain except for a few resistant colonies.

the air might inhibit the growth of the cholera organism and if they were swallowed would exert their antagonistic effects in the intestine. On putting this to the proof he found 11 strains of air bacteria out of 220 from various parts of Versailles and 4 out of 253 from Paris which when cultured at 37° C. inhibited the growth of *V. cholerae*. He obviously tested them on solid media, for he described sterile zones round the colonies varying in radius from a few millimetres to a centimetre, while many others had slighter effects. Most of the antagonistic organisms were like streptococci. He thought the next stage was to see if the faeces of the good inhabitants of Versailles harboured these organisms, but apparently this was never done—possibly Nitsch went home to Poland. Another Pole, Choukevitch (1911), carried on the observations with three of Nitsch's Versailles strains—Gram-positive organisms whose colonies had the appearance of streptococci when grown on agar. He confirmed that if a streak of one of these organisms was planted on an agar surface and *V. cholerae* was planted at the same hour on the following day, growth of the vibrios did not take place within a distance of 1 cm. from the antagonist. He found that the inhibition was not due to acid, that the inhibitory substance developed in broth as well as in solid media, and that it was thermolabile and not capable of filtration. He attempted to establish the antagonists in the intestines of newborn rabbits—animals which Metchnikoff had shown to be susceptible to cholera—but he was not able to demonstrate any protection against ingested cholera organisms, perhaps because he could never show that the cocci had established themselves. This episode is of considerable interest in that it was another early attempt to utilize for medical purposes organisms which produced one or more antibiotics.

Some New Ideas and Procedures

The nineteen-twenties saw the introduction of some new ideas, new procedures, and new antagonistic organisms. In 1921 Lieske published a monograph on the actinomyces, the great majority of which are harmless soil saprophytes. He wrote: "It is easily shown that actinomycetes secrete a specific substance acting extracellularly. There is a fairly wide zone round an actinomycete colony where other organisms are killed. This inhibition is not simply due to exhaustion of the medium by the actinomycete, for colonies of bacteria which

have already grown are lysed and disappear entirely." He studied particularly the effects on killed bacteria, many species of which were lysed, but he also saw lytic and inhibitory effects on live bacteria. He had in mind the possibility of making use of this property in therapeutics in a similar way to that in which pyocyanase had been used but nothing came of this.

Later Gratia and Dath confirmed his observations; they found a streptothrix which was particularly active, and examined many sources for antagonists (Fig. 6). They wrote: "We have

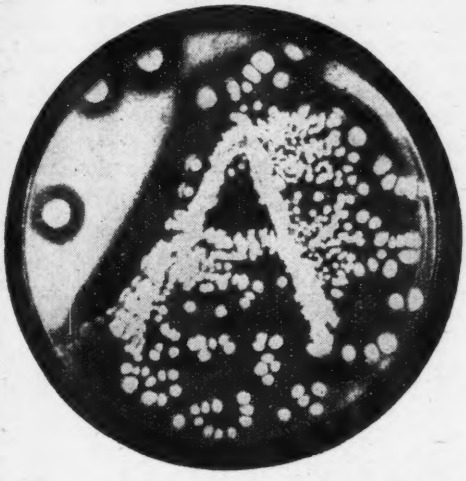

FIG. 6.—Demonstration by Gratia and Dath (1924 and 1934) of the lytic power of a streptothrix. The gelatin contained a heavy emulsion of staphylococci. After 3 days, as shown, there was lysis of the staphylococci round the streptothrix (planted in the shape of an A), and after 7 days the plate had cleared completely.

exposed plates of 2% gelatin in tap-water, incorporating a suspension of a sensitive microbe, such as the cholera bacillus, to air, and to such vehicles as tap-water, drain-water, mud, etc. By this means we have isolated active colonies not only of the usual streptothrix but of other moulds and also of various saprophytic bacilli, both Gram-positive and Gram-negative. As we see it these are examples of a general phenomenon in which a great variety of agents act against many saprophytic and pathogenic bacteria. Among these agents the streptothrices are the most widespread and the most active." Gratia proposed to use the lytic powers of his streptothrix for the purpose of preparing vaccines. He dissolved certain organisms, such as the staphylococcus, by means of the product of the streptothrix later called actinomycetin, and produced what was known as a mycolysate. This mycolysate was used for immunizing purposes with, it was claimed, results which were better than those obtained by ordinary vaccines. This idea, however, was not even then new, for Nicolle had performed experiments on animals in 1907 in which he showed that immunity could be produced by using the products of *B. subtilis*. Working in 1923, 1924, and 1925 with the same group of spore-forming organisms, Kimmelstiel, Much, and Sartorius had described the production of a lysin from certain *B. mycoides* strains which they named *B. cytolyticus*. By means of this organism lysates of numerous pathogenic organisms were made and produced commercially under the name of "sentocym," and there are clinical reports of their efficacy in such conditions as *Bact. coli* infection of the urinary tract. As well as using it for producing vaccines, Much had the idea of employing the lytic substance as a chemotherapeutic agent, for he wrote:

"The material for injection obtained by the activity of *B. cytolyticus* contains, firstly, a solution of a particular bacterium, that is a colloidal dispersion of the bacterial content—the lysate. Secondly, it contains the dissolving agent—the lysin. This lysin acts in three ways. Firstly, the bodies of the highly reactive *B. cytolyticus* (Much) form a powerful unspecific therapeutic stimulus. Secondly, the lysin acts on the infecting bacteria in the body as a lytic agent and so frees the body directly from the bacteria. We are dealing here, therefore, with a real 'bacteriolytic' power, which is quite different from the hypothetical power of a serum. This power is clearly and unambiguously shown *in vitro*. Thirdly, products from the bacterial bodies destroyed *in vivo* by the lysin are added to assist the stimulating action of the lysate."

In 1925 Zukerman and Minkewitsch found on a plate of pseudo-diphtheria bacilli an accidental contaminant which was antagonistic to their growth. This is yet another example of an accidental contaminant which was noticed because of its inhibitory properties and subsequently isolated and examined. The organism was *B. mesentericus vulgatus*, and it showed antagonism only to diphtheria and pseudo-diphtheria bacilli, not to staphylococci, *Bact. coli*, *Bact. typhosum*, or a number of dysentery bacilli. Just as pyocyanase and the substance of Lode had selective antibacterial action, so this material showed the phenomenon to a very marked degree. The antibacterial substance was demonstrated to be mainly bactericidal—a 1% solution of a broth filtrate being potent enough to kill *B. diphtheriae* in 24 hours. Experiments were done with animals to see if the filtrate, the activity of which was heat-stable, was capable of affording protection to guinea-pigs against *C. diphtheriae*; but no sure results could be obtained, and the authors did not report them in detail. In 1925 Rosenthal also contributed work on the bacteriolytic powers of *B. scaber*, another spore-forming organism.

In 1929 Fleming noticed an accidental contaminant—a mould this time—which was also lytic to certain pathogenic organisms. The substance it produced was found to appear in the broth and to belong to the slow-acting type of antiseptic. It had marked differential powers of inhibiting bacterial growth. The crude metabolism liquid was shown not to be more toxic to animals than broth in which nothing had grown. No experiments were performed on experimental infection in animals, but the suggestion was made that the substance would be useful for dressing septic wounds in man, and indeed some were so treated without, however, any very striking results. The clinical possibilities of this antibiotic were not pursued at that time.

About that time, too, the explanation was forthcoming of why pyocyanase had largely dropped out of use in the clinic, as Lode complained when he wrote in 1929, referring to the views of Sonnenberger (1913): "In spite of these recommendations papers on therapy with pyocyanase have almost disappeared from recent literature. This is unjustifiable, so experienced clinicians like Herzog the otologist inform us, for the base of suppurating or very dirty wounds is cleaned quickly without damage to the tissues." There is no doubt that active pyocyanase had often been prepared in the laboratory, and probably commercial preparations had been active at first; but Wagner in 1929 and Kramer in 1935 showed that what activity commercial pyocyanase possessed at that time was almost entirely due to its contained phenol—a good example of what may happen to commercial biological preparations without the necessary strict control. How powerful active pyocyanase can be is shown by the recent work of Rake *et al.* (1943), who found that it inhibited the growth of *Str. pyogenes* at a dilution of 1 in 24,000.

In the nineteen-twenties another idea which has never been thoroughly examined was introduced by Schiller (1924a, 1924b, 1925a, 1925b, 1927; Schiller and Giltscher, 1928). In a series of papers he brought forward evidence that yeasts and possibly other organisms could be made to produce substances that were antagonistic to certain bacteria by growing them on media in which the only source of nitrogen was the bodies of the bacteria it was desired to antagonize. He claimed that in such experiments yeasts developed a thermolabile inhibitory substance which might be of value in therapeutics. He called the phenomenon "induced" antagonism, in contradistinction to the "natural" antagonisms we have so far been considering, and it seems likely that much more might be done to develop this line of research.

During the nineteen-thirties examples of antibiotic effects continued to be recorded and some suggestions made for therapeutic employment of the products; for example, Weiland in 1936 described the action of *B. mesentericus* against diphtheria bacilli and proposed that culture material from this organism would be at least as good as pyocyanase.

The Position in 1939

We may perhaps summarize the position at the end of the nineteen-thirties thus. A very large number of observations on antibiosis had been recorded from 1877 onwards—some of

the most striking have been considered, but there were a great many more—and from all this work certain proposals had emerged for the use of micro-organisms in therapeutics:

1. The replacement of a pathogenic organism by another and less harmful organism—for example, "*Bact. termo*" to replace the tubercle bacillus in the lungs; staphylococci to replace diphtheria bacilli, and pneumococci to replace meningococci, in the throat; lactic acid bacilli and *Bact. coli* to replace organisms infecting the intestine.

2. Artificial immunization by one organism to protect against infection by another.

3. The use of lytic substances from one organism for the preparation of soluble vaccines of other species—for example, Much's sentocym and Gratia's mycolysates.

4. The use of soluble bacterial or mould products by parenteral injection to treat established diseases—for example, pyocyanase in anthrax, and Vaudremer's extracts of *Aspergillus fumigatus* and Rappin's extracts of *B. subtilis* in tuberculosis. These were examples of true chemotherapeutic use.

5. The use of soluble bacterial or mould products as a topical application for the treatment of local infections—for example, pyocyanase and penicillin broth.

The Crystalline Polypeptides

In spite of an enormous bulk of work extending over 70 years, much of which has not been considered here, up to the end of the nineteen-thirties no antibiotic substance from a mould or bacterium had found an undisputed and permanent place in medicine. But in 1939 important papers were published by Dubos in which he described the isolation of a very active selective antibiotic from a spore-forming soil bacillus, *B. brevis*. The work embodied in these and subsequent papers comprised systematic and detailed investigations, both chemical and biological, and reached a new level in the scientific investigation of antibiotics. The active substance, now called tyrothricin, was separated into two crystalline polypeptides—gramicidin and tyrocidine—the first of which was particularly active against Gram-positive organisms such as pneumococci and streptococci. With this material animal experiments were performed, and it was shown that, although it was very toxic when introduced intravenously, it protected mice infected with pneumococci intraperitoneally to a high degree when small amounts of the drug were introduced into the peritoneal cavity even some hours later than the infecting dose of organisms. The work was then carried to the clinic, and it was clearly established that in some ways this antibiotic was superior to any known chemical antiseptic for local application to sites infected with susceptible organisms. A very considerable literature has now collected on the subject, some of it reminiscent of early pyocyanase work, and it seems probable that gramicidin will not occupy a permanent place in medicine only because of the discovery of less toxic substances soon after its introduction. Such, however, is the repercussion of one branch of science on another that *x*-ray analysis of these crystalline polypeptides and others like them may help to elucidate the structure of proteins. Already much interesting chemical and biochemical work has been done on these substances.

Penicillin

We have seen that the idea of using an antibiotic for parenteral injection into an infected animal had been brought forward for pyocyanase by Emmerich and Löw, for the products of *B. subtilis* by Rappin, for *B. mycoides* by Much, and for *Aspergillus fumigatus* by Vaudremer, but no antibiotic had been unequivocally shown to possess curative properties when so used. In 1940 the first observations on penicillin from Oxford were published (Chain *et al.*) which distinguished clearly this antibiotic from any of the preceding ones, for up to this time the real significance of penicillin had escaped detection. There are a large number of substances of both synthetic and natural origin which kill bacteria. Unfortunately most of them also damage animal tissues. Lister clearly realized this in the early days of antiseptic surgery. As his biographer, Godlee (1917), wrote:

> "The antiseptic system aimed at destroying the germs by chemical agency, either before they gained access to a wound at the time of its infliction, or so soon afterwards that they had no chance of developing and multiplying; and, this end having been attained, it further aimed at preventing by means of chemical antiseptics the access of germs to the wound until the process of healing was complete. The problem as thus stated sounds simple, and so it appeared to be in the early days. But, even from the first, one great complication was apparent. Lister recognized the power of the living tissues to control the growth of micro-organisms, and the importance of not interfering with the vitality of the tissues by any harmful agency whatever. Therefore he felt bound to reduce the strength of the chemical substances which he employed to the minimum consistent with efficiency."

It has been seen how pyocyanase was used as a local antiseptic, and later it was proposed that penicillin broth could be used in the same way. The experimental work at Oxford disclosed, however, that an extract containing penicillin could be made which was of such incredibly low toxicity that it could be injected intravenously or by other parenteral routes in amounts far greater than those required for therapeutic effect without producing any sign of toxicity (Abraham *et al.*, 1941). It could thus circulate in the blood and body fluids in sufficient quantity to destroy sensitive bacteria in combination with the natural

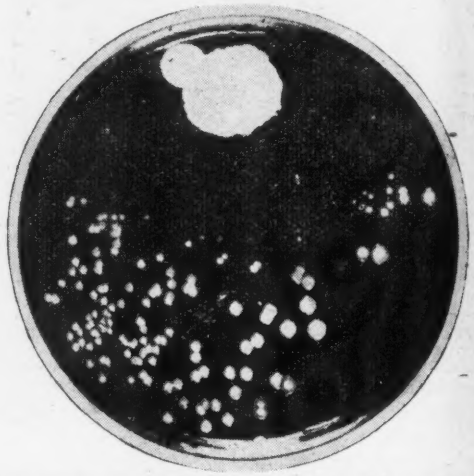

FIG. 7.—Photograph of a culture-plate showing the dissolution of staphylococcal colonies in the neighbourhood of a penicillium colony. (Alexander Fleming, *Brit. J. exp. Path.*, 1929, **10**, 226.) (Reproduced by permission of the Editors of the *British Journal of Experimental Pathology*.)

body defences without the least damage to those defences or to other tissues.

This clear-cut experimental demonstration of its chemotherapeutic action was the starting-point of the serious endeavour to apply penicillin in the clinic. By the time these observations were made—in 1940 and 1941—it had already been clearly established that synthetic chemicals of the sulphonamide group could be absorbed into the blood stream in sufficient concentration to deal in a spectacular manner with many infections—for example, lobar pneumonia and meningococcal meningitis and certain streptococcal infections such as puerperal sepsis. It was natural to suppose that these compounds would be of the greatest use in war surgery, and they were employed in large amounts both by mouth and by local application to wounds throughout the war. Probably the sulphonamides have been instrumental in diminishing generalized sepsis by preventing the spread of streptococci from wounds into the blood stream, but it was evident fairly early in the war that their use was not the final solution of the treatment of the suppurating wound or even for prophylaxis. The main reason for this is probably that there exist many inhibitors of the antibacterial action of the sulphonamides and that some of these are present in pus and in the breakdown products of tissues. It was therefore of the greatest good fortune that it had been discovered by 1941 that extracted penicillin was fully active in the presence of pus and tissue breakdown products, for it was the knowledge of this fact in conjunction with knowledge of the diffusibility and lack of toxicity of the extracts which made much of the subsequent work to bring the substance into clinical use appear worth the great effort necessary. From the results of experimental work it seemed more than likely that penicillin could be applied with success to septic infections involving pus formation either by injecting it into the blood stream or by local administration. These expectations have been fully borne out by subsequent clinical investigations, which have shown clearly that infection by sensitive organisms can be checked and overcome. An

additional encouragement lay in the fact that the staphylococcus was highly sensitive to penicillin, thus bringing into its therapeutic field a group of infections which were outside the range of any of the sulphonamides then known.

In the use of penicillin in war surgery there has been an evolution. Attention was at first directed to the relatively small proportion of wounds which had become severely and chronically infected, as it was thought that here the small amounts of penicillin then available would have the greatest effect. But the slow and sometimes disappointing progress in these deeply infected wounds drew attention to the fact that the use of penicillin in warfare involved not one consideration but two—not only the best methods of application but also the best time at which to make the application. So a move was made to the forward hospitals, and wounds of a few days' standing, even though septic, were treated by appropriate surgery, including suture, and penicillin applications, and in this way an attempt was made to prevent serious and chronic sepsis ever developing. Later, when penicillin became more plentiful —at the time of the invasion of North-West Europe—it was possible to give penicillin prophylactically soon after wounding. Though many factors have contributed to the excellence of the surgical results obtained in the later years of the war few will, I think, now dispute that penicillin has played an important part.

You would not wish me to describe again the reasoning on which the procedures for the use of penicillin in surgery were founded, but on this occasion it may be appropriate to point out that the position has now been reached when a very great measure of success can be claimed in the treatment and prophylaxis of sepsis for a combination of aseptic methods—that is, of methods not associated with chemicals and of antiseptic methods which involve their use. The really important point with regard to the latter is the recognition that there exists an antiseptic beyond the dreams of Lister—an antiseptic which can be applied through the blood stream as well as by appropriate local means, one immensely powerful against many pathogenic organisms but at the same time harmless even to very delicate tissues, and one fully active in surroundings where the sulphonamides fail.

An Outburst of Research

With the demonstration that at least one of the antibiotics was not simply an antiseptic, as had previously been supposed, but a chemotherapeutic agent in the sense in which that word is now used, a great outburst of research has taken place all over the world. Workers who had abandoned the field years ago again became interested, and it was not long before the air was filled with the cries of those wishing to join the new experimental gold rush and to secure what had previously been abandoned. In these days it is uncommon to peruse a medical journal without finding a record of some new organism with antibiotic powers or the re-examination of one already known. One of the most piquant has recently appeared: a mould isolated from human hair in New York has been found to produce a red dye which is active against *Bact. coli*. What a chance for journalists, who have already shown something of their imaginative powers in connexion with penicillin, to wax lyrical and romantic if this substance should ever be of therapeutic importance and it should be disclosed that it was a woman's hair ! I cannot help feeling that Lister would be mightily amused to see the army of eager enthusiasts now stretching out suppliant Petri dishes in the hope that they will receive some miraculous therapeutic manna. Some workers think that the energy spent in the search for new antibiotics would be more profitably spent in investigating more minutely the action of those already known, with the object of understanding bacterial metabolism better. This may be true, but, on the other hand, the search for more antibiotics with a chemotherapeutic action may be of service to surgery ; for, successful as the application of penicillin has been not only in war but also in civilian surgery, it cannot be said that all forms of sepsis can yet be controlled. Penicillin is ineffective against the Gram-negative bacilli, such as *Ps. pyocyanea*, *Bact. coli*, and *Proteus*, and these organisms can still cause mild suppuration in wounds free from streptococci and staphylococci. There is in addition the possibility of resistant strains arising, and though this has so far happened relatively rarely with penicillin, alternative drugs which will do for penicillin-resistant strains what the latter has done for some sulphonamide-resistant strains would be a valuable acquisition. The search, perhaps less urgent now the war has ended, goes on, and already a most interesting compound, streptomycin, has been isolated by Waksman and his collaborators (Schatz, Bugie, and Waksman, 1944) from an actinomycete—one of the group of organisms which, it will be recalled, was first shown to produce antibiotics by Lieske in 1921. Streptomycin is a base which is remarkably non-toxic to animals and can be given in large doses in much the same way as penicillin, and it is active against some of the Gram-negative organisms such as *Bact. coli* and *Ps. pyocyanea*. It has been shown, in mice, to have true chemotherapeutic action against infection by *Ps. pyocyanea* and other Gram-negative organisms, and it has been tried clinically—though with no clear-cut success—in typhoid fever and other infections. It has also some effect, both *in vitro* and experimentally *in vivo*, against the tubercle bacillus. As yet little has been reported of its use in surgery. Besides this most interesting compound this group has furnished streptothricin (Waksman and Woodruff, 1942), similar to but more toxic than streptomycin, and proactinomycin, a substance isolated by Gardner and Chain (1942) from an accidental contaminant. This last substance acts against Gram-positive organisms, and has recently been shown to have chemotherapeutic properties against peritoneal infection by the streptococcus when given by mouth (Florey, Jennings, and Sanders, 1945). It may be a measure of the progress in the chemotherapeutic field that this fact has not excited us greatly, for eleven years ago, before the discovery of the sulphonamides, it would have been considered almost miraculous. Some recent unpublished work which the authors permit me to mention illustrates clearly how very careful one has to be in assessing the powers of antibacterial substances. These three substances from the actinomycetes are all bases, and Dr. Abraham and Dr. Duthie find that their antibacterial power is considerably altered by relatively small changes in the reaction of the medium in which they act. Thus at *p*H 8 their activity is many times greater than at *p*H 7, and at *p*H 7 more than at *p*H 6. When it is realized that autolysing tissues and pus are quite acid it becomes clear that *in vitro* observations carried out in media at *p*H 7.4 or thereabouts may give far too optimistic a picture of what these substances are capable of achieving in the body.

Another development which illustrates fully the great importance of the biochemist in this field of work has recently been furnished by Doisy and his colleagues (1945). Biochemists and bacteriologists working together have isolated four crystalline substances from the organism on which so much sustained work was done in the early days—*Ps. pyocyanea*. These substances, which the workers call Pyo I, II, III, and IV, act powerfully against Gram-positive organisms and appear to have little toxicity to animal tissues, but experiments with infected animals have not yet been reported. It would be intensely interesting if this organism which has been worked on for so long should at last yield a substance of permanent value in therapeutics.

The field for research is obviously vast, and at the moment much attention is being applied to the possibility of discovering an antibiotic which will be effective against tuberculosis. That such a possibility exists no one will deny, but the urge to fame is such that I greatly fear there will be many unreliable and premature publications on this subject. The exploration of the possibility that skin fungal infections and protozoa may be susceptible to some antibiotics has only just begun—with what prospects no one can yet say.

Conclusion

I hope I have been able to make clear that the present happy position in the prevention and treatment of sepsis has not been reached by a flash of insight on anyone's part, but that we at Oxford made a choice from among the many antibiotics known and had the great good fortune to be able to show for the first time that an antibiotic could also be a chemotherapeutic agent.

One great lesson which a survey of work on antibiotics should have taught us is that real and continuous progress and understanding can be obtained only by the collaboration of people

with various outlooks and experimental accomplishments, and, in particular, that little progress can be made without first-class biochemical work. The other important point is that steady and rapid progress is possible in the clinic only when the clinician has firmly based experimental foundations on which to work. It is easy to exaggerate the importance of antiseptic drugs in surgery, and I have indeed noticed signs of restiveness among surgeons when misguided people have wished to attribute all their success in dealing with septic conditions to penicillin. I have always maintained that the introduction of such drugs demands better, not worse, surgery, and a better understanding of inflammatory processes. And it is, I think, quite clear that penicillin and substances like it will only be used with maximum effect if surgeons have a real appreciation of the properties of the drugs—not only what they can do, but also what they cannot do and the reasons for their limitations.

Perhaps I may close with some words of Lister's written in 1867: "And I may take this opportunity of warning some of your readers that they must not expect carbolic acid to act like a charm; but that, whether they employ this agent or some other of analogous properties, it is only by the light of sound pathology, and strict attention to practical details, that they can hope to attain in their full measure the magnificent results which the antiseptic treatment is capable of affording."

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NEUROLOGICAL COMPLICATIONS OF INFECTIVE HEPATITIS

BY

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Cerebral changes in fatal cases of infective hepatitis have often been recorded. They have usually taken the form of coma, delirium, convulsions, and incontinence—symptoms which are common to the clinical picture of acute hepatic necrosis of any origin. Other neurological findings are, however, less well recognized, and it is the purpose of this paper to discuss various signs observed independently in cases of infective hepatitis by four persons in different parts of Assam and Bengal during 1944.

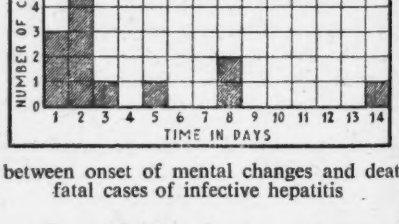
The neurological findings have been protean, but it is possible to dissect out four main groups. First, the picture commonly found in the fatal case, which is too familiar to need detailed description. Secondly, a symptom-complex comprising generalized or localized muscular rigidity with increased tendon-jerks, sometimes but not invariably associated with extensor plantar responses; in addition chloreiform movements have been observed, and in one case a Parkinsonian tremor. Thirdly, a group of cases with large focal haemorrhages into nervous tissue, which may or may not produce localizing signs. Fourthly, those cases showing signs of peripheral neuritis for whose direct relation to the onset of infective hepatitis there is good evidence.

The Common Type

This group may be illustrated by a "type" case:

S., aged 28, developed anorexia, followed by jaundice and bile-stained urine, on Dec. 19, 1944. He remained deeply jaundiced, and on Jan. 3, 1945, became drowsy and apathetic. The liver dullness was diminished, but there were no objective signs in the nervous system. On Jan. 4 he became alternately stuporous and maniacal. The urine contained no leucine or tyrosine crystals, and the cerebrospinal fluid, under a pressure of 100 mm., contained 60 mg. of protein per 100 c.cm. He died on Jan. 5. Necropsy showed characteristic changes of acute hepatic necrosis in a liver that weighed 750 g. The only demonstrable change in the brain was that of congestion of small vessels in the white matter.

There is nothing unusual about this case, but it illustrates well the short time that often elapses between the first onset of mental change and death. A histogram of this interval in 14 cases is shown in the accompanying figure.



Interval between onset of mental changes and death in 14 fatal cases of infective hepatitis

Some degree of mental depression is very common in infective hepatitis. Any increase in this depression or any behaviour change should be treated as a danger sign and as an indication for such protective therapy as is available.

The Striatal and Pyramidal Group

Nine cases have been observed in this group. The distribution of the relevant physical signs is shown in the accompanying Table. The following are brief histories of Cases 4 and 6.

Case 4.—A British private aged 30 was admitted on May 25, 1944, complaining of anorexia and dark urine for two weeks and jaundice for 10 days. On examination he was afebrile and deeply jaundiced.

The liver was palpable and tender. Otherwise there were no abnormal signs. There was bile in the urine, and a diagnosis of infective hepatitis was made. There was normal progress up to May 31, when the jaundice was barely discernible, except that he had been found sleep-walking on the nights of May 29 and 30. He was not, so far as he knew, in the habit of sleep-walking. On May 31 he was drowsy all day, but took food and answered when spoken to. In the evening he became distant and unapproachable, and the same night was incontinent of urine. Examination revealed slight spasticity of both arms. On June 1 he was in coma. There was deep jaundice and the liver was still palpable. Both arms were spastic, adducted, and pronated. Both legs were spastic, adducted,

Distribution of Striatal and Pyramidal Signs in 9 Cases of Infective Hepatitis

Case No.	Muscular Rigidity	Increased Jerks and Clonus	Involuntary Movements	Extensor Plantar Responses	Outcome
1	+	−	−	−	Death
2	−	+	−	−	,,
3	−	+	+*	−	,,
4	+	+	+*	−	,,
5	+	+	+†	−	,,
6	+	+	−	+	,,
7	−	+	−	−	Recovery
8	+	+	−	+	,,
9	+	+	−	−	Death

* Frank choreiform movements. † Parkinsonian tremor of hands.

and extended. Plantar responses were flexor. All tendon-jerks were very brisk and there was sustained left ankle clonus. The pupils were not dilated and gave normal reactions ; corneal reflexes were absent; fundi were normal. During the day these physical signs remained unchanged, but there appeared in addition choreiform movements of both arms, more marked on the right side, and purpuric spots on both upper arms. He died early on June 2 with more purpura on the right hand and a sharp terminal rise of temperature. Lumbar puncture revealed no abnormality. A white blood count showed a slight polymorph leucocytosis. There were no malaria parasites in repeated blood slides. There was no albuminuria.

At necropsy there were numerous subpleural, subpericardial, and mesenteric petechial haemorrhages. The liver was not diminished in size; it had a red-and-yellow blotchy appearance and the architecture was clearly destroyed in patches. The brain showed no macroscopic abnormality, there were no malaria parasites in brain smears, and there was no bile staining of brain tissue.

Histology.—Liver:—Marked degeneration of the parenchyma with a moderate amount of fibrosis and cellular reaction. Proliferation of bile ducts and formation of new parenchyma very pronounced. Evidence of degeneration even in newly formed epithelial cells. Brain:—Generalized oedema of white matter. No perivascular cuffing or general increase in cells. No cellular degeneration. Virchow-Robin spaces widely separated from the vessels. A small extravasation of red cells into the brain tissue from one vessel in the midbrain.

Case 6.—A British N.C.O. aged 24 was admitted on Sept. 16, 1944, complaining of anorexia for five days and dark urine for three days. On examination he was moderately jaundiced and there was bile in the urine. There were no other abnormal signs. Infective hepatitis was diagnosed. On Sept. 19 he became drowsy yet restless. Jaundice was more intense and the liver dullness was diminished. There were no abnormal signs in the nervous system. Later in the day he became delirious. On Sept. 20 he was still more deeply jaundiced and in coma. The pupils were dilated but not fixed, and the fundi were normal. There were bilateral extensor plantar responses, but no alteration in muscular tone and no involuntary movements. Later in the day his temperature rose slightly. Both legs became spastic and were held adducted and extended. There was bilateral sustained ankle clonus. The temperature rose steadily to 105° during Sept. 21, coma deepened, and the physical signs remained unchanged. He died early on Sept. 22. There was never any purpura. The urine contained no albumin, and repeated blood smears were negative for malaria.

Necropsy revealed endocardial petechial haemorrhages. The liver was small and flabby with a shrunken capsule, and the cut surface was blotchy yellow and red. The brain showed no macroscopic abnormality, no bile staining, and negative smears for malaria parasites.

Histology.—Liver:—Extensive parenchymatous degeneration with marked leucocytic infiltration. Brain:—Generalized oedema, especially of the white matter. Perivascular lymph spaces greatly enlarged. No haemorrhages. No degenerative changes. Occasional vessels in the caudate and lentiform nuclei showed a few round cells, perivascular, but the appearances were not suggestive of an encephalitis.

In all fatal cases the liver showed the characteristic pathological changes of acute or subacute hepatic necrosis. Histological sections of the brain are available in five cases, including the two detailed above. They consisted of scanty round cells, perivascular around the basal ganglia, in the meninges, and in one case in the subcortical white matter. A greater or lesser degree of cerebral oedema was always present, and extravasation of small numbers of red blood cells throughout the brain was sometimes observed. These changes are in alignment with those found by Baldwin Lucké (1944) in his excellent analysis of fatal hepatitis in the U.S.A., except that in one of our cases lymphocytic infiltration was found in the subcortical white matter as well as in the meninges and basal ganglia.

The pyramidal and striatal signs may be accounted for by one of three theories: a mutation of the infective hepatitis virus, giving it a neurotropic character ; a specific attack upon the basal ganglia and pyramidal systems by products of autolysed liver cells ; or by toxins from the gut which are allowed to pass through an incompetent liver.

The virus theory is improbable on account of the following facts. The onset of striatal signs coincides with a sudden increase in liver damage and not with the onset of the infection. The cellular reaction round the small vessels of the brain is very slight and can hardly be considered to indicate a meningoencephalitis. It is conceded, however, that the tempo of the disease, once cerebral changes have appeared, is so swift that more dramatic histological changes may have no time to appear ; again, it is possible that special methods of staining which were not available might have demonstrated more extensive damage.

The chemical theory is more likely. It is interesting to note the relation between striatal and pyramidal disease and diffuse liver damage in such diverse conditions as kernicterus (Hawksley and Lightwood, 1934), umbilical infection (Biemond and van Crefeld, 1937), Kinnier Wilson's disease (Greenfield, Poynton, and Walshe, 1924), Laennec's cirrhosis (Waggoner and Malamud, 1941), and such metabolic disturbances as haemochromatosis and the lipoidoses (Brouwer, 1936). Many of these conditions are characterized by diffuse liver disease of a chronic nature in which there is no active autolysis of liver cells. It is for this reason that we prefer the last explanation—namely, that of poisoning of nervous tissue by the passage of intestinal toxins through an inefficient liver.

The pathogenesis of these signs is probably the same as that of the " type " picture, but the association between hepatic and striatal damage in so many diseases is too frequent to be pure coincidence and demands further investigation.

The Haemorrhagic Group

It is the rule rather than the exception to find diffuse petechial haemorrhagic phenomena in fatal cases of infective hepatitis as in any disease causing liver failure. We have observed three cases which are worth noting, since they showed massive haemorrhages—into the caudate nucleus in one case, and forming a subdural haematoma in the other two.

Case A.—A British officer became ill on July 1, 1944, with nausea and vomiting followed by jaundice. On July 14 he became unconscious and incontinent; the plantar responses were both extensor, but later in the day the right response became flexor. On July 18 there was a petechial rash and he bled extensively from ulcers on his scalp, shoulder, and wrist. Necropsy on July 22 showed the common hepatic changes of necrosis; the brain contained an area of softening lateral to the right lateral ventricle and a large haemorrhage into the right caudate nucleus. Histology demonstrated small extravasations of blood into the meninges and into the Virchow-Robin spaces in the pons; there was also slight perivascular round-cell infiltration.

Case B.—A sapper developed infective hepatitis on July 26, 1944. There were small subconjunctival haemorrhages and haematemesis. Blood slides showed a few benign tertian rings. On Aug. 3 he suddenly lapsed into coma and died the same day. Necropsy showed a right parietal subdural haematoma. The surface vessels of the brain were congested and there were petechial haemorrhages throughout the white matter. Brain and spleen smears showed no malaria parasites, and the liver changes were those of acute hepatic necrosis.

It is suggested that in the first case the lesion (possibly a venous thrombosis) giving rise to the area of softening occurred at the same time as the extensor plantar response was observed. The caudate haemorrhage which gave no signs was probably

terminal. The subdural haematoma both in the case quoted and in the other was silent.

This is a small and somewhat artificial group which should be regarded aetiologically as a gross manifestation of the general haemorrhagic tendency in liver failure.

Peripheral Neuritis

In the Arakan in 1944 there was a small and short-lived outbreak of peripheral neuritis which coincided precisely in time with the development of jaundice in each case. This outbreak will be reported elsewhere by Byrne, who considers, with some reason, that the symptoms and signs of peripheral neuritis were due to the emergence of a neurotropic element in the virus of infective hepatitis. We have observed one such case:

A British private aged 38 complained of a painful heel on July 26, 1944. He had cellulitis of the heel, which cleared up by Sept. 20. There was no open ulcer. He then noticed difficulty in walking (the difficulty involved both legs), and a week later he developed infective hepatitis. Jaundice was persistent, the weakness of the legs became gradually worse, and the arms became weak also. On Nov. 12 (the last day on which he was seen, as he developed amoebic dysentery and had to be evacuated) examination showed normally reacting pupils, no cranial nerve lesion, general weakness of the upper limbs with absent tendon reflexes, and general weakness of the legs with absent knee- and ankle-jerks. There was wasting of the dorsiflexors, with blunted touch sensation on the legs and slight Rombergism.

It seems doubtful whether the peripheral neuritis in this case was causally related to the infective hepatitis. Hurst (1943) observed similar cases in Gallipoli in the 1914–18 war, and it is by no means certain that they did not represent the simultaneous occurrence of two fairly common diseases.

Summary

The neurological complications of infective hepatitis are reviewed.

Four main groups are illustrated by cases observed in Assam and Bengal in 1944.

It is concluded that neurological changes may appear in association with infective hepatitis due to a number of causes, most of which are still a matter for speculation.

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A TRICK TEST TO DETECT NIGHT-BLINDNESS "MALINGERERS"

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The tests that have so far been evolved to examine scotopic vision—pentagon, hexagon, and the various adaptometers—are all subjective in nature and therefore demand the full co-operation of the subject. From the military point of view this is of little value in eliminating the few genuine cases of night-blindness from the many who simulate the condition. To help in sifting these cases the following test was devised and preliminary experiments have been carried out.

It was thought that if it could be shown that the subject was co-operating fully, then more reliance could be placed upon the subjective results of one of the above-mentioned night-vision tests. A mass of experimental data (Hecht and Williams, 1922; Hecht and Mandlebaum, 1939; McDonald, 1940; Mandlebaum, 1941) indicates that red light stimulates the cones of the retina only and that the rods are unaffected. On this basis, subjects with normal central vision—that is, adequately functioning cones at the macula—should be able to identify a pure red light at minimal intensity even if the rest of the retina is diseased or defective.

Experiments performed by us have shown that the minimum threshold for pure red falls within a very narrow range of intensity of illumination (Fig. 1). An individual with defective night vision but with normal cones—that is, normal macular function—should theoretically be able to see red light within this narrow normal range of minimal intensity. This hypothesis has been confirmed by the few cases of early retinitis

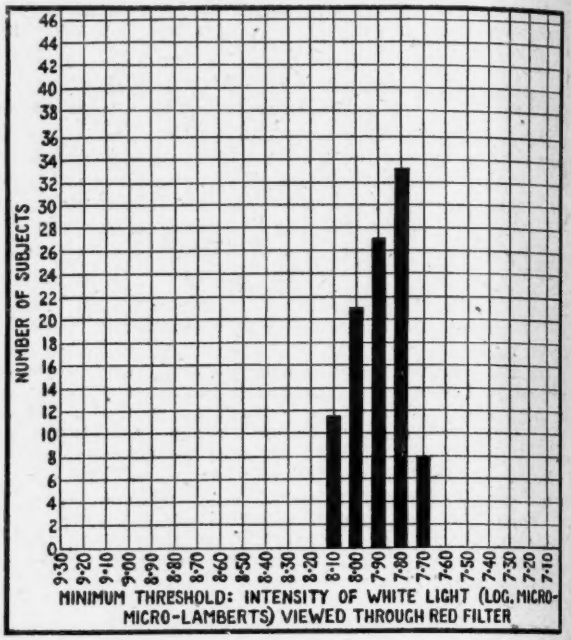

FIG. 1.—Range of minimum threshold for red light of 100 subjects chosen at random and including those with hypermetropic and myopic errors of refraction.

pigmentosa, diffuse choroido-retinitis (maculae unaffected), and advanced glaucoma with very contracted fields but good central vision which we have so far been able to test. One possible exception, suggested by the literature (Wald, Jeghers, and Arminio, 1938; Wald and Steven, 1939; Hecht and Mandlebaum, 1939; Mandlebaum, 1941)—for we have not been able to find such a case—is severe nutritional deficiency (vitamin A), because it is reported that in experimentally induced vitamin A deficiency the cone threshold for white light, and therefore we presume the red threshold as well, is raised. In practice, however, this exception can be dismissed, as this condition cannot arise on Army diets and would, in addition, show other well-marked features. Finally, it is necessary that known cases of night-blindness (familial) with no signs of disease or abnormality of the fundi should be tested to ascertain if such cases fall within the above established normal range of minimal thresholds. This would substantiate the universal application of the test.

Method

The test evolved is briefly as follows. The individual who complains of night-blindness at the out-patient clinic is given a careful routine examination including colour vision, then placed in a dark room, given dark goggles, and left to dark-adapt for 20 minutes. Other patients are seen meanwhile, so that no time is wasted. After 20 minutes' adaptation the "night-blind" patient is asked to state when he can just see the faint red light which is shown him and increased gradually in intensity; as soon as he can see the light it is dimmed and the test repeated a few times until a constant result is obtained. It is simple to deduce whether the patient is co-operating or not. The general procedure of the test suggests to the subject that his night visual ability is being tested, and the malingerer reasons that he must not admit seeing the light until it is very bright; but this, of course, does not tally with the results of the general routine investigation, which shows that he has normal macular function. Further, the several readings

taken often give inconstant results in this type of case, as it is difficult for the malingerer to judge an equal brightness intensity each time.

This test takes up about five minutes of the operator's time. When a result within the normal range is obtained it can be assumed that the patient is co-operating, and one of the tests of scotopic vision can be performed with reliance upon the results.

The apparatus used is Crookes's adaptometer 1944 (Fig. 2) (Godding, 1945), which consists of a screen illuminated by a

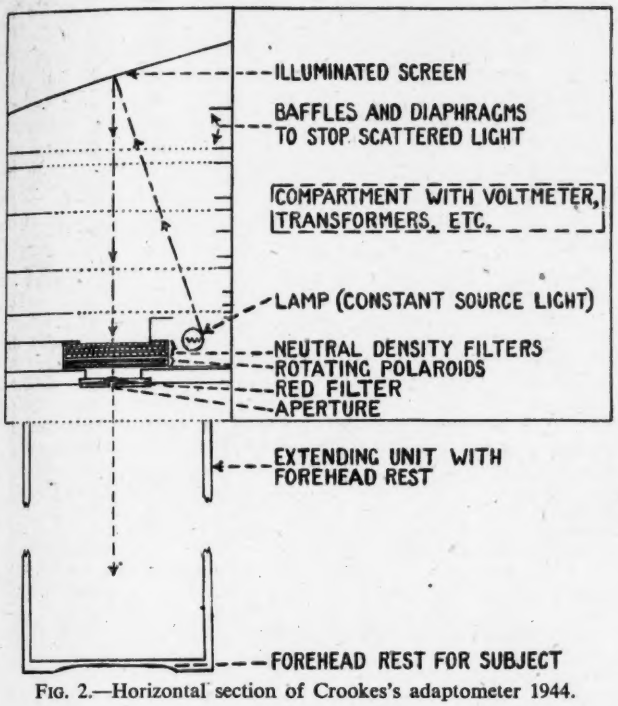

FIG. 2.—Horizontal section of Crookes's adaptometer 1944.

constant source. The intensity exhibited is varied by neutral density filters and rotating polaroids placed between the screen and the observer. This has been calibrated by the National Physical Laboratory. The red filter used is Dufay-Chromex No. 2257, which transmits the longer waves of red light as shown in Fig. 3—i.e., 680 to 720 mμ. Of the various filters tried

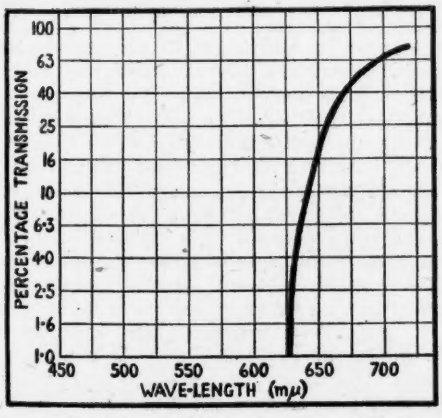

FIG. 3.—Transmission of Dufay-Chromex filter No. 2257.

this was found to be the best, as it "crossed the threshold coloured," the other filters allowing the transmission of shorter waves, which were perceptible as a faint grey before the light was seen as red, thus making it difficult to determine the threshold and giving a wider variation in the normal range. A square aperture, subtending an angle of 1°, diagonally, at the testing distance, is used in front of the filter, and the patient is placed in position and shown this before the test so that he knows where to look and can fix on it. A square aperture is used as suggested by Sloan (1939) to avoid confusion with the vague sensations of light experienced by subjects at threshold values. A 1° aperture at threshold values for red light stimulates only the macula and is not appreciated by paramacular regions.

The subject is first tested without spectacles. It was noted that many cases with high refractive errors gave subnormal results without their glasses and normal readings with them. The value of spectacles in these instances was not only refractive but helped maintenance of fixation, for the esophoria of hypermetropes and exophoria of myopes became manifest in the dark. The more observant of these subjects volunteered that their eyes seemed to have wandered in the dark and that they suddenly saw the red light when they moved their eyes a little to one side. Cases with small refractive errors—less than +3 or −3D—usually gave the same result with or without glasses; or those cases giving a very good result without glasses might give a slightly worse reading, though well within normal, with the spectacles. No doubt the explanation of this is the absorption and reflection by the lenses of some of the incident light.

Illustrative Case Histories

Case 1.—Lieut. A. had complained of night-blindness in 1943, when he was examined at an ophthalmic centre and a very poor result (1/5) was obtained with the Army pentagon test. He was considered bad enough to be excused night duties, but was left in Category A. He was seen in Jan., 1945, after posting to a new unit, the authorities of which considered it incongruous for an officer to be Category A though deemed so night-blind as to be excused night duties. Routine examination revealed no abnormality, but, tested with the red light, he admitted seeing it only when it was very bright and well outside the normal range. Before taxing him with this incongruity he was referred to a psychiatrist—Major McCarthy, R.A.M.C.—who diagnosed an extremely abnormal state bordering on psychopathy.

Case 2.—Lieut. B. had always had difficulty in the black-out. When with the B.L.A. he found that one relatively dark night he was unable to see to drive, and had to employ a driver, who managed quite easily. The fundi showed numerous small retinal cysts (Blessig) which reached to well behind the equator. The red test gave a normal result. The dark-adaptation curve showed a slow rate of adaptation and a final threshold definitely higher than normal. This officer was considered to be handicapped in the dark; he was suitably categorized.

Case 3.—Gnr. C. had difficulty in getting about at all in the black-out. Routine examination, red test, and dark-adaptation curve were all normal. Further questioning revealed the story of an accident during the first month of the black-out, when he had run into the back of a stationary lorry with no tail light and had severely bumped his right eyebrow. He then realized that the pain which he had been experiencing above his eyes in the black-out was a "memory" of this accident. Reassured of his perfect sight, he went away feeling much happier. His medical officer was advised to refer him to a psychiatrist if his symptoms recurred. A follow-up four months later elicited that he had not complained about his eyes and was performing his duties satisfactorily.

Case 4.—Pte. D. had difficulty in driving in the black-out. Red test and dark-adaptation were normal. Close questioning revealed that he disliked driving at any time. He was referred to the psychiatrist, who reported a severe anxiety neurosis necessitating institutional treatment.

Case 5.—Sign. E.'s eyes were normal. The red test showed non-cooperation. Further questioning revealed nothing of note. His medical officer was notified to disregard all complaints of visual defect at night.

We wish to thank Brig. Sir Stewart Duke-Elder for permission to publish this paper, and we are grateful to the Crookes Laboratories for the loan of their adaptometer.

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The first issue of a new American periodical, *Quarterly Review of Psychiatry and Neurology*, is now in preparation. The editor-in-chief is Dr. Winfred Overholser, and the quarterly will be published by the Washington Institute of Medicine, 1720, M Street, N.W., Washington 6, D.C. (annual subscription $9.00).

GENERAL ANAESTHESIA AND THE TEMPERATURE OF INHALED GASES

BY

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It would appear that for long there have been misconceptions regarding the temperature of gases and vapours as presented to the patient for inhalation. There have also been differences of opinion on the advisability of warming these gases. The idea seems particularly to have prevailed that ether vapour must necessarily have the objectionable quality of coldness. Gwathmey (Hewitt, 1912), in 1908, stressed the need for raising the temperature of the vapour in order to prevent the loss of heat by the patient incurred in warming up a cold vapour in his air passages. Seeling (Hewitt, 1912), on the other hand, in 1911, concluded that the warming of anaesthetic vapours was unnecessary. In 1918 Pembrey and Shipway advocated warming the vapour, with a view to maintaining the patient's body heat; Shipway's warmed-ether apparatus was introduced at that time. Minnitt and Gillies (1944b) do not consider that warming the gases is essential when administered by modern methods, but "if, however, the old method of insufflation is used, in which there is no reflux of exhaled gases, and especially if the gases are blown through ether at a fairly high flow-rate—say 8 litres per minute—then the resultant mixture passing into the lungs is of a sufficiently low temperature to be detrimental, and some form of heating is advantageous." Minnitt and Gillies (1944c) state: "There can be little doubt that the blowing of a cold mixture of gas, oxygen, and ether at a high rate of flow—10 litres or more per minute—into the patient's lungs is a major cause of the high incidence of post-anaesthetic respiratory complications, which are frequently noted when continuous-flow machines of the Boyle type are used, or, perhaps one should say, abused."

The closed circuit and the Oxford inhaler are both commended by some because they maintain a *warm* anaesthetic mixture for inhalation. Open ether, when ether is administered by a perhalation system, is almost invariably associated with the appearance of patches of snow on the gauze, and doubtless this suggests to many the inhalation of a "frosty anaesthetic vapour." Referring again to Minnitt and Gillies (1944a), when the subject of shock and anaesthesia is under discussion a factor contributing to shock production is given—namely, "heat loss by exhalation, and particularly if the patient has to breathe through ether on an open mask."

To test the various opinions expressed on this subject the following observations were made. Temperatures are expressed in degrees Fahrenheit, and were taken with a sensitive chemical thermometer. When obviously unimportant, fractions of a degree were not recorded.

The ether in a drop-bottle, the ether, and the ether vapour and air mixture in the ether bottle of a Boyle machine were all at theatre temperature—73°. The face-piece was removed from the 30-in. wide-bore delivery tube of the machine, and the thermometer was inserted well within the face-piece end of the tube. When a mixture of 1 litre of oxygen and 5 litres of nitrous oxide was allowed to flow the temperature was 73°. The mixture was allowed to pass over the ether in the ether bottle—temperature 73°, and then to bubble through the ether in the ether bottle—temperature 73°. Two litres of oxygen and 10 litres of nitrous oxide were now allowed to bubble through the ether; in 10 minutes the outer surface of the bottle was coated with ice; the temperature of the mixture gradually fell to 69°, at which it remained stationary.

Temperatures of the mixtures as actually inhaled by the patient during anaesthesia were recorded in a large number of cases. *Open ether:*—The mask was a simple one covered with 16 layers of surgical gauze. The temperature was taken by inserting the bulb of the thermometer through a small aperture in the gauze, so that it was situated in the mixture of air and ether vapour within the mask. It was noticeable that the lower temperatures were recorded when the patient's respirations were vigorous, and the higher temperatures when the breathing was quiet. The lowest temperature recorded was 80° and the highest 91°. *Boyle's machine:*—Gas, oxygen, and ether with limited rebreathing; the temperature of the mixture within the face-piece was 90 to 97°. *Closed circuit:*—Temperature within the face-piece was 92 to 95°.

The conversion of liquid nitrous oxide into a gas and of ether into a vapour are both associated with loss of heat, but the gas and vapour apparently at once acquire the temperature of their environment, and when inhaled are above room temperature, owing to their admixture with the warm exhaled gases. The results obtained in estimating the temperature of the inhaled mixture when the perhalation method is employed may give rise to some surprise. Presumably the important factor here is that 16 layers of gauze, with the entangled air, form a very efficient non-conductor of heat. It can be demonstrated that if a mask of this description is placed in position over the patient's face the temperature of the air within the mask rises in about two minutes from 73° (room temperature) to 87°, and in about the same time becomes saturated with water vapour. The formation of patches of snow is a phenomenon similar to the freezing of the skin with the ethyl chloride spray.

The body heat of the patient during anaesthesia was also studied.

The patient's temperature was taken in the axilla, first immediately before induction and then at frequent intervals—as a rule every 10 minutes. In short simple operations variations in temperature were negligible. A fall of from one to two degrees was noted in the more severe cases; occasionally there would be a rise of temperature. Duration of anaesthesia was in this respect an unimportant factor as compared with the nature of the surgical procedures. A particular study was made of the patient's temperature where open ether was the method of administration throughout the operation. There was quite definitely no fall in the patient's temperature on completing induction, and the temperature was maintained at the pre-induction level during lengthy operations not involving severe surgical measures. Colporrhaphies, for example, lasting 50 minutes, gave records such as 97.2°, 97°, 97.2°, 97.4°, 97.4°. As an example of a more severe operation, a panhysterectomy with difficult adhesions, duration 80 minutes, showed variations in temperature which were obviously associated with the surgical manipulations; before and after induction the temperature was 97.8°, falling after 50 minutes to 96.5°, and rising to 97.2-97.5° on closing the peritoneum. Partial gastrectomies and resections of bowel gave very similar figures. The most marked fall of temperature (from 98° to 96°) was recorded when the operation was an osteotomy of the femur, duration 90 minutes—anaesthesia, cyclopropane and ether, closed circuit.

Discussion

The maintenance of the patient's body heat during anaesthesia is an important matter, but it would appear that in this respect the influence of the temperature of the inhaled anaesthetic gases has been unduly stressed. According to the physiologists (Starling, 1941), in the adjustment of the temperature in man, regulation of the heat loss plays a greater part than heat production. Of the heat loss 80% is by the cutaneous circulation and evaporation of sweat, 11% by evaporation of water in the expired air, 4% in the liberation of carbon dioxide from the lungs, and 5% in warming *food* and air from 58° to 99° F. It is obvious that a very small percentage will be lost in warming up the air from 75° to 99°. Buxton (1914), quoting Kappeler, states that the body temperature is reduced 0.5 to 2.7° under ether. The body temperature is, however, susceptible to many different influences: it falls with loss of blood, and markedly so, in surgical shock. Many factors during an operation may influence the patient's temperature. It, moreover, is obvious that the warming of gases not only is unnecessary but is impracticable. The warmed gases would be at room temperature by the time they reached the face-piece.

Conclusions

It is not necessary to warm the gases or vapours when conducting inhalational anaesthesia.

Open ether should not be condemned as presenting a "cold vapour" to the patient for inhalation.

There is no evidence that general inhalational anaesthesia is associated with loss of body heat.

The variations in the patient's temperature during operation are due to other factors.

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FOREIGN BODIES IN THE BRONCHI AND OESOPHAGUS IN CHILDREN

BY

B. M. L. ABERCROMBY, M.B., B.S., D.L.O.

Out of a number of cases of foreign bodies in the bronchi and oesophagus which have passed through my hands during the past five years it is thought that the following five are worth reporting as they illustrate various points which may materially affect the ease or difficulty with which these bodies may be removed.

Foreign bodies are less common in the bronchi than in the oesophagus. On account of the more rigid structure and small diameter of the bronchus, especially in children, removal is always more difficult in these cases. Removal is also complicated by the fact that, unless the foreign body is impacted, it tends to move up and down the lumen of the bronchus with respiration and cough, thus causing irritation of the mucous membrane and producing a varying amount of oedema, which may ultimately occlude any view of the offending article. The lumen of the oesophagus is considerably larger than that of the bronchus, even in a child, and the walls are lax and distensible, providing more space for manipulation, which considerably facilitates the removal of a foreign body.

Case I

A male child aged 5 was admitted to hospital on April 24, 1944, with a history of having inhaled two glass beads three days previously. One of these he had subsequently coughed up before admission, and it had been retrieved. Several severe attacks of coughing were accompanied by partial respiratory obstruction, alarming enough for the practitioner in charge of the case to send the child to hospital. On admission the patient appeared comfortable and continued so as long as he remained in one position. Any change, however, tended to bring on an attack of coughing, which at times was accompanied by signs of dyspnoea. This was explained, and subsequently demonstrated at the time of removal, by the bead being forced up into the trachea and impinging on the lower surface of the vocal cords. There were no signs of collapse of the lung, and *x*-ray examination revealed the bead situated in the left main bronchus.

Operation was performed under chloroform anaesthesia and the 25-mm. Irwin Moore bronchoscope passed down into the bronchus. The glass bead was easily visible in the bronchus, and was propelled up and down the lumen with each act of respiration. Attempts to seize the bead were found difficult for two reasons: (1) the extremely smooth surface of the bead, which allowed of no firm grip; and (2) although the bead was not impacted, there was hardly enough room to pass the forceps ends over it on either side in order to grip it. After several unsuccessful attempts the patient began to come round and the tube was withdrawn. While this was being done he coughed, and the bead was seen to be propelled into the trachea up against the lower surface of the cords, but before the forceps could be reintroduced it had been sucked back again into its original position. Under deeper anaesthesia the tube was reintroduced and a cotton-wool swab soaked in adrenaline held against the mucous membrane around the bead for a few seconds. This resulted in shrinkage of the tissues, and after one or two more attempts it was possible to secure a firm grasp of the bead and withdraw it into the lumen of the bronchoscope, and then to withdraw the whole. Further examination showed no damage to the bronchial mucous membrane. Beyond a slight laryngitis for a few days, the child suffered no ill effects, and was discharged about a week later.

Case II

A boy aged 3 was admitted to hospital on Jan. 9, 1945, at 3.30 p.m., having swallowed an open safety-pin at midday. Immediate *x*-ray examination showed the pin, open end downwards, lodged in the oesophagus just below the post-cricoid region (Fig. 1).

The interest in this case lies in the fact that the safety-pin had passed into the oesophagus point downwards—a very unusual occurrence, but one which considerably facilitates the removal. It is interesting to compare this case with the one reported by Mr. Ritchie Paterson in the *Journal* of Feb. 17, 1945, in which the pin entered the oesophagus point upwards. Mr. Ritchie Paterson described the dangers associated with the removal of a pin in this position and the dexterity required to effect this without perforating the oesophageal walls, with the possible consequence of mediastinitis.

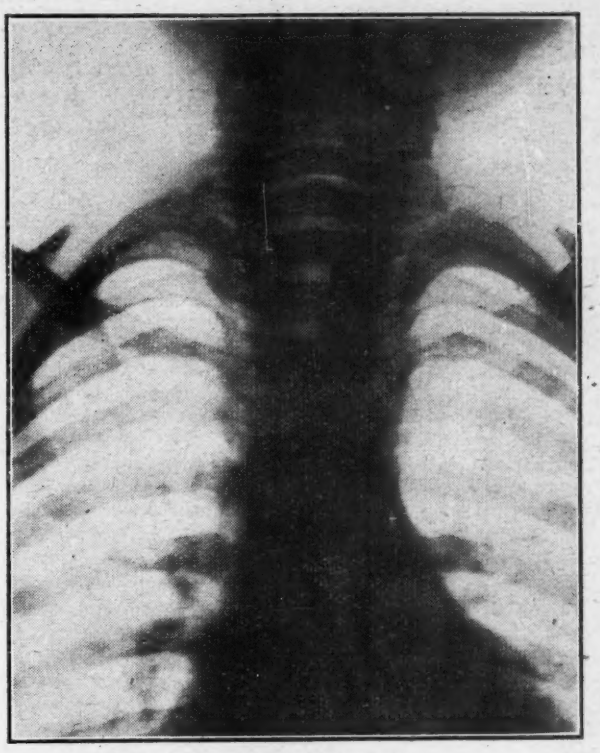

FIG. 1

In the present case, on introduction of the oesophagoscope under general anaesthesia, the pin was seen lightly poised on the posterior wall of the oesophagus. The point had presumably been caught between two folds of the mucous membrane without actually perforating the tissues. It was an easy matter to seize the rounded end with the forceps and withdraw the pin into the lumen of the tube and then to remove the oesophagoscope and its contents in one act. There was no damage to the tissues, and the whole operation, including demonstrating to the nursing staff, took at the most five minutes. The patient was sent home next day, none the worse.

Case III

On Jan. 9, 1943, a girl aged 14 was admitted complaining of cough and pyrexia for two weeks before admission. Clinical examination showed signs of collapse of the right lung. Skiagrams of the chest revealed a very large paper-clip, with a round head and two prongs, lodged head downwards at the bifurcation of the right main bronchus (Fig. 2). Each prong of the clip was later found to be 3/4 in. long. A preliminary bronchoscopic examination was done under chloroform anaesthesia, as it was thought that a good deal of pus and oedema might be present and preventing a view of the clip, thus interfering with its removal by direct vision. This surmise proved correct, and even with gentle pressure with a long wool-carrier the foreign body could not be felt. It was therefore decided to defer any further attempt at removal until this could be done under the *x*-ray screen. Owing to the disproportion in size between the diameter of the bronchus and the length of the prongs, it was obviously impossible to manipulate the clip so as to remove it with the points dangling and thus avoid damage to the mucous membrane. The only hope was to attempt to seize the prongs about half-way down their length, close them as much as possible and draw them into the lumen of the bronchoscope.

The patient was laid on the *x*-ray table and chloroform anaesthesia induced. A further examination through the bronchoscope proved the clip still invisible by direct vision. Under the screen, however, it was clearly seen, with the head firmly lodged down the right main bronchus. A pair of forceps was then passed down the tube to the clip and several attempts were made to secure a hold of both prongs together, but these were not successful. It was felt that the clip must be removed at all costs, even at the risk of damage to the surrounding tissues. A firm grip of one of the prongs was therefore

obtained and gentle traction applied. This was accomplished only after turning the child on her right side. Watching all the time through the screen, the clip was pulled up for about an inch. It then stuck, and it was seen that one prong had caught in the tissues. Further gentle traction showed that this prong was acting as a fulcrum round which the head was rotating. Traction was therefore continued, and the prong, with its point embedded, was seen to be gradually bending at its centre. Traction was continued till the

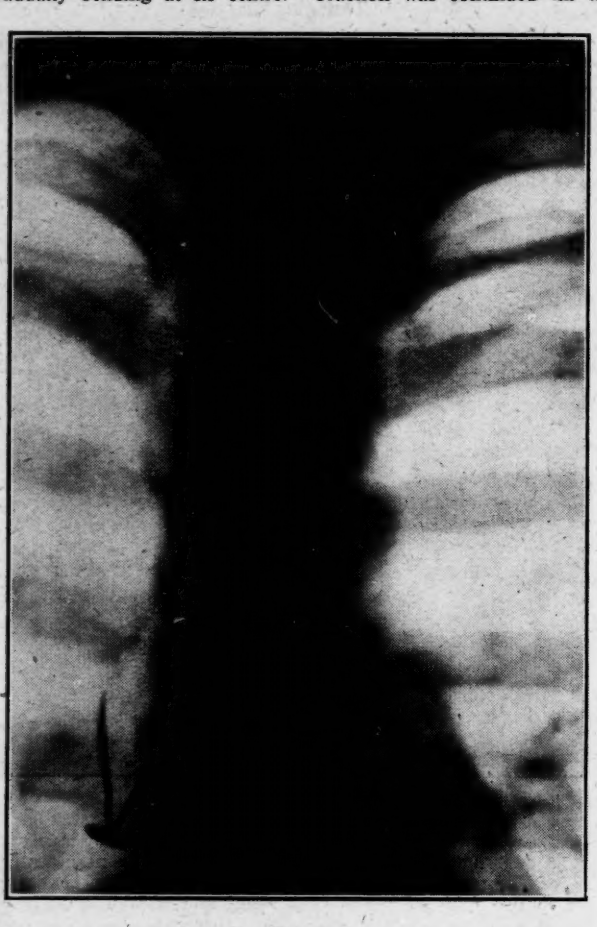

FIG. 2

prong was bent double and no further progress was being made. Then, still with a firm grip on the first prong, a slight downward push was made in an attempt to dislodge the point. This was successful, and by a stroke of luck the now-doubled prong entered the lumen of the bronchoscope when further traction was applied. Thereafter it was a comparatively easy matter to draw the whole clip into the tube and withdraw the latter and its contents in one movement. The proceedings were viewed the entire time through the *x*-ray screen, and it would have been impossible to remove the foreign body without the vision thus afforded.

The patient subsequently developed, not surprisingly, a complete collapse of the right lower lobe and a swinging temperature for a while, but with the help of sulphapyridine and breathing exercises she made a satisfactory recovery, and was discharged from hospital on March 25, 1943, to attend the chest clinic as an out-patient.

Case IV

A boy aged 12 was admitted on April 30, 1945, with a history of having swallowed or inhaled a bent pin which he had been holding between his teeth in an effort to straighten it. The patient was not distressed in any way and there were no abnormal physical signs. *X*-ray examination showed an apparently straight pin lodged in the commencement of the right main bronchus. An ordinary white steel pin similar to that involved was seen to have some ornamentation of an orange-coloured composition material around the head.

The child was placed on the operating table and chloroform anaesthesia induced. On introducing an Irwin Moore bronchoscope into the right bronchus one-half of the pin could be easily seen. With a little manipulation the ornamented head end came into view on the right, and it was seen that the pin was in fact bent into a "V," as stated by the child, though this was not apparent on the films. Having ascertained the shape and position of the pin, forceps were introduced and a firm grip obtained as near the point as possible, which was then drawn into the lumen of the tube. The decorated head of the pin impinged on the rounded end of the bronchoscope. This was then gently withdrawn without any difficulty till the subglottic region was encountered, when there was a slight hitch; but a little manipulation sufficed to withdraw the pin past the vocal cords, after which there was no further difficulty.

Other than a moderately severe laryngitis for a few days after the operation there were no ill effects, and the child was discharged fit on May 7.

Case V

A baby aged 18 months was admitted at 10 p.m. on May 19, 1945. The mother said she thought the baby had swallowed a piece of bone or button—"there were plenty of bits on the floor." Immediate *x*-ray examination revealed a shadow about the post-cricoid level, but it was difficult to be sure whether it was in the air or the food passage. From an oesophagoscopy performed under general anaesthesia it was evident that there was no foreign body in the oesophagus. Bronchoscopy was then attempted, but, owing to a misunderstanding as to the age of the child in the first place, the smallest-sized bronchoscope was not available, and the next size was too large to pass through the glottis without undue force, which would have damaged the cords. A brief glimpse was obtained, however, of the upper part of the trachea, but no foreign body could be seen. The child was breathing comfortably and was in no way distressed, so it was decided to postpone further examination till another skiagram could be taken the next day. This was done, and a lateral view showed a small pointed body at the level of the sixth cervical vertebra, but it was still not clear whether it was in the bronchus or the oesophagus. A barium meal was therefore given, and it immediately became clear that the offending article was in front of the oesophagus in the left bronchus, possibly exerting slight pressure on the oesophagus.

The baby was taken to the theatre and chloroform anaesthesia induced. The smallest-size Negus bronchoscope was then introduced through the larynx with the aid of a Mackintosh electrical laryngoscope. Owing to the small lumen of the tube the view was very restricted, but by keeping well to the left side of the trachea the bronchoscope passed easily into the left main bronchus. After sucking away some mucus there appeared to be a small dark hair-line across the lumen of the bronchus, which could not be sucked away and was presumed to be the foreign body, although no adequate view could be obtained. A very fine pair of forceps was introduced down to the site and gentle palpation performed, when something hard could be defined. The forceps were opened and manipulated till it was felt that a grip on the hard body had been obtained; traction was then exerted until the forceps ends and their contents were within the lumen of the bronchoscope. The latter and its contents were then withdrawn in one movement. On examination the foreign body was seen to be an irregular piece of button of some composite material, approximately half an inch long and a quarter-inch in diameter.

Beyond slight pyrexia for a few days the baby suffered no ill effects, and was discharged from hospital on May 25.

I am indebted to Dr. W. E. Crosbie, medical superintendent of Alder Hey Hospital, for permission to publish the above cases, and to Dr. Bennett-Jones for the anaesthetic in Case III and Dr. Derham in the four remaining cases. I have also to thank the radiographer, Mr. Lucas, for the *x*-ray films and prints and his valuable assistance in screening the patient in Case III.

AN UNUSUAL CASE OF ERYTHROBLASTOSIS FOETALIS

BY

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AND

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The possibility that erythroblastosis foetalis may sometimes be due to a heterospecific pregnancy (i.e., to maternal anti-A or anti-B iso-agglutinins) has been considered by Wiener (1943), and Boorman, Dodd, and Mollison (1942, 1944) have reported three cases in which this seemed to have happened. A fourth and rather striking case has been investigated by us, with the following findings.

Clinical History

Mrs. A., a primigravida aged 24, attended the antenatal clinic at the City Hospital, Nottingham, on Jan. 28, 1944, and was seen by one of us (J. B. C.). Her L.M.P. was Sept. 29, 1943, and her fundal height at 14–16 weeks corresponded with the period of amenorrhoea. She was found to have an intermediate degree of

pelvic contraction, but otherwise her general condition was satisfactory. When aged 9 years she had had scarlet fever complicated by acute rheumatism, but was nursed at home and was off school for three months, apparently making a complete recovery.

She attended the ante-natal clinic regularly throughout and, apart from the failure of the head to engage at the 36th–38th week, her condition was entirely satisfactory and normal in every respect. Arrangements were made to admit her on July 3, 1944, three days before the expected date of confinement, for investigation of the pelvic contraction. She was admitted in labour to hospital on July 1—approximately at term. The foetus was in the vertex left occipito-lateral position and the head poorly flexed and mobile: no hydramnios was present.

On admission skiagrams for foeto-pelvic disproportion were taken and, as no disproportion seemed to be present, it was decided that she be given a trial labour. After a first stage of labour lasting just over 22 hours, and a second stage of two hours, it was found that the head remained arrested in the transverse diameter of the pelvis at the level of the ischial spines.

Under spinal anaesthesia a live mature male infant with gross hydrops foetalis was delivered by manual rotation and forceps. It weighed 7 lb. 2 oz. and was 17½ in. in length. After birth the infant's colour and cry were both very poor, and remained so despite lobeline injected into the umbilical vein. The infant died 25 minutes later.

The placenta and membranes looked normal, did not show any of the typical macroscopic features generally associated with such a foetus, and weighed 21 oz.—a normal ratio in weight to the infant. At delivery blood was taken from the umbilical cord for investigation, the results of which are given below.

Necropsy of the infant showed gross oedema of all the viscera, with much free greenish fluid in all the serous cavities. The lungs were surprisingly well aerated though markedly oedematous, and apparently had been expanded as fully as was possible under the circumstances. The liver and heart appeared normal, but the spleen was large, though normal in shape. No evidence of injury to the meninges or of intracranial haemorrhage was found, but the brain was very oedematous and much more friable than normal. The thymus was also much larger than normal, although its consistency was not apparently altered. Pieces of liver, thymus, spleen, sternum, and a lymph node were sent for histological examination, and these showed the very characteristic histological picture of erythroblastosis foetalis.

The patient made an uninterrupted recovery and was discharged on July 17. She was seen at the post-natal clinic on Aug. 15, when her condition was satisfactory from an obstetric point of view. She was asked to return in four weeks' time for further serological investigations, but has so far defaulted.

Serology

Father's blood—Group A Rh-negative. Mother's blood—Group B Rh-negative. Foetal blood—Group A Rh-negative. No irregular iso-agglutinins were found in the mother's serum, but her anti-A titre—determined against A_1 cells of average sensitivity by the method of Taylor and Ikin (1939)—was 16,000,000. Anti-A was looked for in a sample of foetal blood (obtained from the cord) and was found present to a titre of 128. Despite the presence of this anti-A in the foetal plasma, the foetal erythrocytes were not agglutinated; we think that this may have been due to the gradual elimination of the more sensitive foetal red cells (i.e., those containing substantial amounts of A substance) by the powerful maternal anti-A iso-agglutinin, since the foetal red cells were relatively insensitive to anti-A sera, and were agglutinated by the maternal anti-A only to a titre of 1024. The husband's cells were agglutinated to a titre of 250,000 by the mother's serum.

On Aug. 8, five weeks after delivery, the maternal anti-A titre was 4,000,000. Unfortunately, it was not possible at the time to determine the A subgroups of husband and baby owing to lack of the necessary sera. As the patient failed to attend again no further serological investigations were possible.

In view of the extraordinarily high maternal anti-A iso-agglutinin titre, and of the presence of Anti-A in the foetal blood, it is difficult to avoid the conclusion that this was a case of erythroblastosis foetalis due to heterospecific pregnancy.

Our thanks are due to Dr. W. R. Harvey, Royal College of Physicians Laboratory, Edinburgh, for the histological report, and to Dr. C. L. Crawford Crowe, medical superintendent, City Hospital, Nottingham, for permission to publish this case.

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Medical Memoranda

Full-term Extra-uterine Pregnancy associated with Extensive Prolapse

The following case shows many points of interest in addition to its rarity. There was complete lack of symptoms pointing to an early emptying of the uterine contents into the abdominal cavity. Toxaemia occurred at 36 weeks' gestation. The persistent transverse lie and extensive prolapse were aids to the diagnosis.

Mrs. P., 7-para, aged 41, first reported to the ante-natal clinic when four months pregnant. Her past obstetrical history was as follows: the first two children had been born prematurely at eight months, the first surviving 7 weeks and the second 2 days; she then had four normal pregnancies and labours, all the children being alive and healthy.

At her first attendance at the clinic her only complaint was of "dropping of the womb." On examination the fundus was enlarged to the size of a 16-weeks pregnancy, and on vaginal examination an extensive prolapse, with slightly eroded cervix, was present. The cervix was disinfected, pushed up, and a large ring pessary inserted. The date of her last period was rather uncertain; but there had been no irregular bleeding or pain, and the estimated date of delivery worked out at about the middle of March.

The pregnancy progressed, but the ring pessary was unsatisfactory because it failed to keep the prolapse in position and failed to stay in the vagina for any length of time. As the cervix was now more ulcerated and bleeding, it was decided to admit the patient to hospital. Improvement occurred after admission on Jan. 19, 1945, and while she was in bed the prolapse was not evident except after a bowel action. As the patient had another two months to go before full term, a Napier's pessary was fitted and she was discharged with suitable instructions. She was then very much more comfortable, but had to be readmitted a month later—Feb. 19—with toxaemia of pregnancy. There was gross pitting oedema of feet and legs, the B.P. was 182/110, and the urine contained a trace of albumin. After a week's rest in bed the B.P. had fallen to 140/88 and the urine was clear. The oedema had improved but not quite disappeared.

The foetus at this stage was lying in a transverse position high up in the abdomen. When version was attempted the foetus immediately reverted to its former position. An *x*-ray examination was carried out and the following report obtained: "This is a single pregnancy, transverse presentation, spine upwards, the head being opposite and above the level of the left iliac crest. The biparietal diameter is estimated at 3.6 in."

It was suspected that we might be dealing with an extra-uterine pregnancy or that the placenta might be occupying the lower uterine segment. However, before further investigations could be carried out the patient started to have violent abdominal pain, with vomiting and rapid rise in pulse rate. The pain seemed to be localized at first to the right iliac region, and there were no palpable uterine contractions; but the pain and tenderness rapidly became generalized over the abdomen, together with rigidity of the rectus muscles.

A laparotomy was obviously indicated, and when the patient was anaesthetized with cyclopropane the contour of the abdominal swelling was seen to be quite irregular, and, the muscles being relaxed, foetal parts could be felt very easily. An incision was made in the midline below the umbilicus, and a thin-walled bluish sac presented at the wound with the foetus plainly visible inside. A live male baby was delivered; but, unfortunately, in spite of all efforts at resuscitation, he died about an hour later.

The umbilical cord was about 18 in. long and its distal half was divided into two separate vascular bundles, which entered the placental mass separately. The latter had an extensive attachment in the pelvis, the peritoneal surfaces of the bladder, caecum, enlarged uterus, and broad ligaments being involved. Separation of the placenta, tentatively tried at one spot, was easily effected, and the whole placental mass was removed much in the same way as an adherent placenta is removed from the uterus. No attempt was made to pick up bleeding-points, but the whole bed from which the placenta had been removed was packed with a length of wide ribbon gauze, the end of this being brought out through the lower part of the abdominal incision. The packing was removed 24 hours later under gas anaesthesia, and the opening through which it came was closed.

The patient had a happy convalescence and was able to go home 18 days after her confinement.

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G. E. Morris (*New Engl. J. Med.*, 1945, **233**, 180) states that multiple ulcers of the legs are often due to infestation of the clothing with body lice, especially in the middle-aged. Search for each parasite therefore should be made in all suspicious cases.

Reviews

PUBLIC MEDICAL CARE

Public Medical Care: Principles and Problems. By Franz Goldmann, M.D. (Pp. 226. $2.75 or 18s. 6d.) New York: Columbia University Press; London: Oxford University Press. 1945.

The object of this book cannot be better stated than in its own words: "an attempt to give a complete picture of public medical care as a social movement." The author, as associate clinical professor of public health at Yale and lecturer in the New York School of Social Work, is well qualified to make this attempt. Mr. Gradgrind used to say, "What I want is facts." This book would have satisfied him, for it is full of relevant facts derived from almost every civilized country. Great Britain is freely drawn upon for both facts and historical derivations. Equally important is the critical examination of these facts and of the tendencies they disclose. Public medical care in the U.S.A. has had a long and tortuous history, as it has had here. Dr. Goldmann traces its American development back to the English Poor Law system, through the poor-house, the lunatic asylum, the clinics arising from charitable dispensaries, the rise of the general hospital, and the gradual transfer of responsibility from voluntary agencies to larger and larger political units. This is a familiar picture to British students of the subject, and Dr. Goldmann's comparisons and analogies will be found useful everywhere. Running through the U.S.A. picture are, however, complications from which we have been happily spared, caused by the existence of 48 States, jealous of their independence and only reluctantly consenting to transfer responsibility to the Federal authority even where central control is obviously needed.

The familiar factors—free choice of doctor, means tests for insurance, whole- or part-time doctors, group practice, etc.—are all there, and the author deals with them judicially. There is a good deal of the "on the one hand—on the other hand," but it is evident that on balance Dr. Goldmann is impressed with the administrative convenience and the smaller liability to abuses of the salaried doctor. He is emphatic that the administration, at all events, of medical care should be in the hands of salaried medical superintendents, and has some revealing remarks on the still existing tendency in some areas to put "political pets" into these positions.

As regards health insurance, he is against any "means test," which he says has the disadvantage that it discourages the more deserving from applying, often till it is too late to prevent serious complications or life-long disability. Summing up the various methods of dealing with medical insurance, he is of opinion that the Russians have the only logical plan, though he realizes that it is often impossible to make any progress if one insists on strict logicality—and this certainly applies in the U.S.A. He grants that "under such a system most or all of the members of the health professions would become civil servants, and that private practice if allowed to continue would have to rely on the demand and purchasing power of a relatively small fraction of the population. Those [medical care functions] owned by the non-governmental organizations, if legally permitted to operate, would be faced with constant financial difficulties and would gradually disappear. "The bearing of this forecast on problems now facing the British profession will not be overlooked. The book can be cordially recommended to all who desire to study these problems with as wide a background as possible.

THE TISSUES OF THE BODY

The Tissues of the Body. An Introduction to the Study of Anatomy. By W. E. Le Gros Clark, F.R.S. Second edition. (Pp. 388; illustrated. 21s.) Oxford: At the Clarendon Press. 1945.

Notwithstanding the continuance of the great war from 1939—the year when *The Tissues of the Body*, by Prof. Le Gros Clark, was first published—until 1945, the date of issue of the second edition of this book, a very considerable amount of important research in anatomy has been accomplished. This work has been concerned with problems having a distinct practical bearing, and includes, among other researches, special studies on the regeneration and repair of striated muscle and on the nerve- and vascular-supply of muscle, and also detailed descriptions of the vascular patterns and innervation of blood vessels in animals. The solution of some of these problems had been in progress before 1939, and much of the recent work recorded or referred to in the new edition has arisen from questions which have emerged as a direct outcome of the initial stages of these investigations—for example, the restoration of function of muscles or nerves deprived respectively of their nerve- or vascular-supply. To indicate the type of work referred to, one may mention the effects of re-innervation of paralysed muscles by *sensory* nerve fibres, which has now been conclusively shown by Gutmann to be ineffectual with regard to restoration of their motor function, since atrophy and fibrillation of the muscle proceed in the same manner as in cases in which the motor nerve has been divided and no re-innervation has been attempted. In re-innervation of striated muscle by a *sensory* nerve, even when new sensory fibres succeed in reaching motor end-plates, no connexion is made with these, and sensory fibres are unable to form new end-plates; moreover, stimulation of the grafted sensory nerve does not make the muscle contract. In line with the above example is the demonstration, by methylene-blue injections, of the existence of interconnexions by arterial anastomoses between the branches of intramuscular vessels arising from different sources; though uncertainty exists about the establishment of efficient immediate intramuscular anastomoses with complete regeneration of partially devascularized or injured muscle, which result has lately been demonstrated by the author in conjunction with L. B. Blomfield.

Besides additions to the text recording progress in anatomical research work, a considerable number of excellent new illustrations have been added, and the new edition fully maintains the high standard and practical character of the book.

PERSUASIVE PSYCHOTHERAPY

Active Psychotherapy. By Alexander Herzberg, M.D., Ph.D. (Pp. 152; 12s. 6d.) London: Research Books Ltd., in association with William Heinemann Medical Books.

Nearly all psychotherapists now agree that treatment of the psychoneuroses and sexual maladjustments must rest on an analytical basis, though they are not agreed as to which of the various analytical methods is the best to use. All, however, are conscious of the need to shorten and simplify all existing methods of treating these forms of mental illnesses, since at present such treatment takes a very long time and there are too few psychiatrists to do the work, and far too many patients who require help.

This book is a description of the methods of a very experienced psychotherapist who worked for many years in Berlin, before he had to leave and come to this country, where he found opportunity to continue his work at University College, London. He, in common with others, uses analytical methods as the basis of his treatment, but has devised a method of imposing tasks upon the patient with the idea of shortening the course. These tasks require efforts of will on the part of the patients to which they have been persuaded by the author. It seems remarkable that they should submit to some of these tasks, since they involve such things as giving up an object of love, forcing themselves into situations of which they are terrified, and so on. It speaks much for the persuasiveness of Dr. Herzberg and for the influence he exerts over his patients that he can make some of them carry out these tasks; but it is doubtful if all psychotherapists could aspire to this persuasiveness, or that all patients would find themselves able to carry out the instructions, however they might wish to do so. Nevertheless, when success is attained, there is no doubt whatever that treatment will be very much shortened and more lasting success achieved, since the ability to carry out the tasks almost amounts to cure.

This book is certainly worthy of careful study by all psychotherapists, as there is great promise in Dr. Herzberg's methods if they can be carried out; but those who are not fully acquainted with the difficulties and complexities associated with the treatment of the psychoneuroses and sexual perversions must not expect that a new short method of therapy of universal application has been discovered.

CHEMICAL DISCOVERIES AND ECONOMIC LIFE

This Chemical Age. The Miracle of Man-made Materials. By Williams Haynes. (Pp. 288. 12s. 6d.) London: Secker and Warburg. 1945.

This book is mainly a historical narrative of the developments of chemistry in the economic life of the world. It is written in a bright and racy style such as may well attract readers who are not acquainted with the science or technology of chemistry. Indeed, in the preface, which is entitled "A Word with the Reader," the author says: "I am rather proud how little chemistry there is in this book, which deals with all the new synthetic chemical products." Yet the contents are informative in a high degree to those who are trained chemists, and the volume will be found good by students of chemical history.

The lively and vigorous treatment of the facts of history makes entertaining reading, and the author is to be commended for the thoroughness with which he has explored the available sources of information and for the acute insight which has enabled him to reconstruct pictorially the background and environment of episodes which were critical in the history of discovery. Nothing appears to have been omitted that has or has had an interest to the public who have benefited by chemical discovery. The author has brought his history to the present day, and only finished a little too soon for a discussion of the atomic bomb. His comments on the political and economic effects of chemical discoveries are also shrewd and far-seeing. The discussion covers dyes, synthetic textiles, perfumery and flavouring agents, the sulphonamides and chemotherapy, synthetic rubber, plastics, the influence of chemistry on warfare, and a great number of other subjects, all written in a flowing sequence that makes easy reading. The addition of a glossary under the title "Words that Puzzle," and a very full index, completes a useful book.

A YEAR'S PROGRESS

The Medical Annual, 1945. Edited by Sir Henry Tidy, M.D., F.R.C.P., and A. Rendle Short, F.R.C.S. (Pp. 410. 25s.) Bristol: John Wright and Sons Ltd.

In our last review of this publication we suggested that "the usefulness of the volume as a whole would be much improved by a yearly article under the heading of 'Vital Statistics.'" Whether the Editors acted on this hint, or arrived simultaneously at the same conclusion, an article under this heading by Dr. Percy Stocks now appears and fills "an obvious gap." It is intended to make a review of this subject a yearly feature. As the Editors say, ". . . statistics are invading the life of the medical profession," and statistics is something the doctor, as a practical biologist, does not take kindly to. Yet medicine is becoming more and more a matter of exact measurement, and many an investigator in the past—and in the present—would have been saved endless trouble if he had first of all planned his investigation with a medical statistician at his elbow, and had submitted his observations to the same exact, and exacting, eye before committing himself to print. A very full review on penicillin fills in another gap left in the last *Annual*. "D.D.T." is referred to under the heading of "Pediculosis," but this is hardly a measure of its importance, and we can trace no reference in the edition now before us to its use in the control of malaria. A separate article on insecticides and insect repellants would seem to be needed to put this subject into proper perspective.

The *Medical Annual* has always included a number of illustrations which relieve the bleakness of the printed page, but nevertheless to devote two coloured plates to depict the pinhole meatus, and yet a third to show up the meatal ulcer of infants, seems to be out of proportion; and does the coloured plate demonstrating gangrene of the toes in the condition of "immersion foot" say something that cannot equally well be said in a less paper-consuming way? More informative, we believe, would be the use of isotype illustrations to put statistical matter in a visual form easily grasped by the mind which boggles at numbers and formulae.

Needless to say, this year's *Annual* bears all the usual marks of careful editorship and indexing, and for the many now returning from the Forces to civil life should prove to be an indispensable reintroduction to current medical practice.

Notes on Books

We have received from the Library of the U.S. War Information (1, Grosvenor Square, London, W.) a collection of articles reprinted from *War Medicine* under the general heading *Convalescent Medicine*, the subdivision being devoted respectively to physical therapy, occupational therapy, and emergency treatment of acute war neuroses; also *Tuberculosis*, a collection of articles in two volumes reprinted from various American periodicals, the first volume relating to diagnosis and the second to treatment. These three assembled reprints may be seen at the Library of the British Medical Association.

Clinical Case-Taking, by G. R. HERRMANN (Henry Kimpton; 9s.), of which a third edition has now appeared, gives a compact and detailed account of history-taking and physical examination. Prof. Herrmann believes that a thorough examination of a few patients is a better discipline for the student than a superficial glance at a large number. His book sometimes reads like a drill manual, but there is a great deal to be said for a well-drilled student.

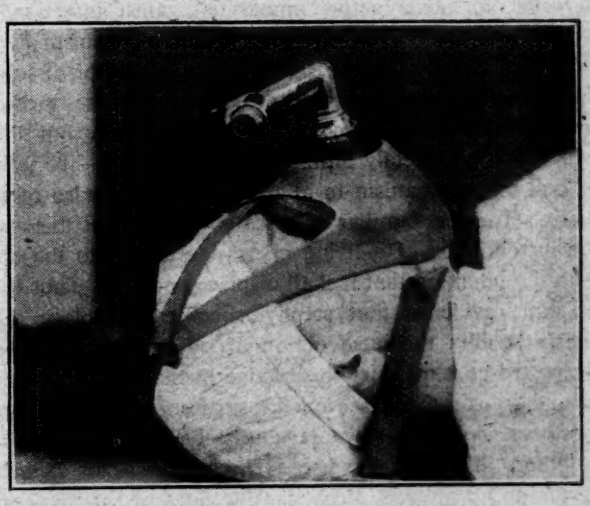
Preparations and Appliances

A COMBINED HARNESS AND TUBING SUPPORT

Dr. HARVEY NICHOL, honorary anaesthetist, Royal Gwent Hospital, Newport, Mon., writes:

Whereas in most cases the harness in common use will retain the face-piece satisfactorily in position, it is sometimes necessary to support the jaw with the hand in order to obtain an unobstructed airway, especially if the face-piece is strapped on tightly enough to ensure a gas-tight fit, as is essential if a completely closed circuit is to be maintained or a "draw-over" principle is being employed. Also the heavy circle type of absorber tubing tends to pull the face-piece out of position.

The apparatus illustrated is composed of a harness made of rubber and a metal L-shaped portion, on which the patient's head rests and to which the harness and tubing are secured. The harness is shaped to fit closely round the chin and face-piece, and has five straps attached to it. The single strap attached to the left side of the harness is threaded through two lugs on the distal end of the horizontal part of the tubing support, which is then placed under the patient's head. The harness is now brought into position under the chin, the free end of the strap



mentioned being fastened to the hook on the right side of it. An airway, if necessary, having been inserted, the face-piece is placed in position, and the two upper straps are crossed over it, each being attached to a pin on the opposite side of the metal vertical. The two lower straps are now pulled horizontally, and when sufficient tension has been applied to give a free airway these are also fastened to the pins already mentioned. Finally the end of one (or each) of the latter straps is made use of to support the tubing by passing it over same and fixing to the pin on the opposite side.

When thus adjusted the jaw is supported, the face-piece held in position with a gas-tight fit whatever the contour of the face, and the tubing does not drag on the face-piece. A good airway is assured, and the patient can be moved into any position without displacing the face-piece or tubing.

The apparatus was made for me by Messrs. Coxeter and Son, Ltd., in conjunction with Messrs. A. Charles King, Ltd.

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MICROBIAL ANTAGONISMS

Few users of penicillin are aware that its development as a chemotherapeutic agent was the culmination of a long series of studies on microbic antagonism going back more than sixty years. Yet this is the theme of the Lister Memorial Lecture delivered by Sir Howard Florey to a large audience at the Royal College of Surgeons on Oct. 11 and printed in our present issue. To many people the idea that one micro-organism may attack another presented itself first in the fully developed and highly refined form of penicillin, but in fact this sort of antagonism was fully recognized even by Pasteur himself, and very numerous examples of this phenomenon were brought to light even between his time and the end of the last century. The lecturer's diligent search of the older literature has revealed a whole series of observations covering a wide area of the field of microbiology which together make a fascinating story.

The use of one micro-organism in an attempt to eliminate infection by another has taken several forms other than that with which we are now generally familiar. Two of them are in reality methods of immunization, involving either the administration of preparations of one micro-organism in order to excite active immunity against infection by another, or the lysis of the specific micro-organism by an extract of another in order to produce a sort of dissolved vaccine—the "mycolysate" of Gratia. These methods involve a different principle and mechanism, but those which depend directly on microbic antagonism—the power of one micro-organism to attack another—are also of two kinds. The less familiar of these is replacement therapy—the introduction of a living micro-organism into the body with the intention that it shall displace another. Thus there is some evidence that staphylococci can displace diphtheria bacilli; the introduction of pneumococci has been suggested as a measure to dislodge meningococci from the nasopharynx; there is even the suggestion that one strain of *Bact. coli* can advantageously be used to eradicate another. This and the use of *L. bulgaricus* or *L. acidophilus* in an attempt to change the flora of the alimentary tract must be viewed in a somewhat different light from the attempted replacement of definite pathogens causing actual infection.

All these proceedings, throughout the long history of such studies, are subsidiary to the use of microbic extracts as antiseptics. That some micro-organisms form substances lethal to or inhibitory to the growth of others has been fully recognized since 1885, and only two years later than this we see the first use of the method of adjoining streaks on plates of solid media, whereon the growth of one micro-organism prevents that of another within a zone in which the antagonizing substance diffuses. Nor was it long before the study of these substances came to embrace moulds. Further still than this, a mould extract was used therapeutically over thirty years ago: Vaudremer employed an extract of *Aspergillus fumigatus* in the treatment of over 200 cases of tuberculosis between 1910 and 1913. It is of peculiar interest that this fungus has quite recently been shown to produce several distinct and very potent antibacterial substances, one of which certainly has the action on the tubercle bacillus *in vitro* which Vaudremer claimed for his extracts, although the possession of this property is unfortunately far from guaranteeing any therapeutic powers. Even the word "antibiosis" was coined as long ago as 1889; hence "antibiotic" as a noun generally descriptive of all such substances has at least the sanction of precedence, and, however inappropriate it may seem, it has evidently come to stay.

The history of the discovery of such substances as this, including the outstanding one of penicillin itself, pursues an almost unbroken course from 1885 to 1940. Many were used therapeutically as antiseptics. One, pyocyanase, became an article of commerce and was extensively used on the Continent for many years. As Florey points out, the new era in antibiotic therapy was ushered in by the work of Dubos on gramicidin. The properties and powers of this substance were systematically investigated. It was chemically characterized and shown to be capable of exerting a systemic effect, although it was too toxic for therapeutic use of this kind. We are led to infer that the example of Dubos encouraged Florey and his colleagues to undertake a similar systematic investigation of penicillin, with what results the world is now well aware. That it proved to have such unheard-of properties—to be not only a true chemotherapeutic agent in the sense of exerting a systemic affect, but almost devoid of mammalian toxicity and singularly indifferent to the adverse conditions which prevent the action of so many other antimicrobic agents—all this was more than anyone could have expected. But these properties were conclusively demonstrated and the foundation laid for production on an industrial scale. It is indeed appropriate that a lecture commemorating Lister should deal with the modern apotheosis of the method which he founded.

Not the least entertaining part of the Lister lecture is that in which Florey describes the present microbiological "gold rush," as he calls the search after new antibiotics which has followed the penicillin revelation. How extensive this search has become and how wide the net is being cast few people can realize who do not closely follow the literature. The most promising genera of fungi have been systematically studied by many investigators, and many antibacterial substances produced by them have been identified. A good idea of their distribution among different genera may be gained from the painstaking studies of Wilkins and Harris,[1] who have now examined 400 species. The same authors[2] have examined 700 species of the larger fungi, while a start has been made on the higher plants by Osborn[3] and on a smaller scale by Huddleson *et al.*[4] Russian authors[5] have discovered mysterious virtues in the vapour emitted by

[1] *Brit. J. exp. Path.*, 1944, **25**, 135.
[2] *Ann. app. Biol.*, 1944, **31**, 261.
[3] *Brit. J. exp. Path.*, 1943, **24**, 227.
[4] *J. Amer. vet. med. Ass.*, 1944, **105**, 394.
[5] *Amer. Rev. Soviet Med.*, 1944, **1**, 237, 239, 242, 244.

crushed onions and other plants, which they call "phytoncides." Also from the Soviet Union come reports[6] of a different and superior gramicidin. An alga is the source of chlorellin, the promising characters of which are described by Pratt *et al.*[7] It is an extraordinary fact that among all these substances, which must now run into thousands, not one has yet been found which improves on the therapeutic performance of penicillin without having some countervailing disadvantage either in toxicity or in the limitation of conditions for its activity. This is a field in which premature enthusiasm should be guarded against. The early therapeutic promise of patulin was not fulfilled, and the astonishing *in vitro* performance of notatin—a far more potent antiseptic under certain conditions than penicillin itself—has no therapeutic counterpart at all. Streptomycin, one of the few reasonably non-toxic fungus products which act on Gram-negative organisms as well as Gram-positive, and recently used for treating typhoid fever by Reimann *et al.*,[8] may perhaps break down, as Florey reveals here, for the simple reason of inactivation by changes in *p*H. There is no knowing where these studies may lead, and the complete conquest of microbic infection may yet be their culmination. The study of such agents has become a task for collaborative effort, in which perhaps the most important of several parts will have to be played by the biochemist. An idea of where such studies may lead may be gained from the work of Hays *et al.*[9] on what used to be called "pyocyanase." From extracts of *Ps. pyocyanea* cultures they have isolated no fewer than five crystalline antibacterial substances, one at least of which has very promising characters.

TRIUMPH AGAINST MALARIA

Now that the war is over, the security silence is being lifted and many scientific developments can be made known to the world at large. After the entry of Japan into the war and the loss of Java with its supplies of quinine, intensive research in the U.S.A. and this country was carried out on the control and prevention of malaria. Some of the results of this work have already been disclosed, others will appear in due course. One of the greatest triumphs was made public early this week, when the discovery of an extremely active new antimalarial compound in the laboratories of Imperial Chemical Industries was announced. The following account is based upon information acquired by the Medical Research Council during the development of this substance.

Early in 1942 a team of chemists and biologists began the search for a new synthetic antimalarial which could replace the lost supplies of quinine and which would prove more effective in preventing and treating malaria than this somewhat imperfect medicament. Antimalarial activity was found in several interesting types of chemical compounds, but no satisfactory result was obtained until 1944, when Mr. F. M. Rose, working in collaboration with Mr. F. H. S. Curd, synthesized a substance which was at first known by the code number M.4888. This was tested by Mr. D. G. Davey upon experimental infections of *Plasmodium gallinaceum* in chicks and on other forms of avian malaria, and was shown to be remarkably active in destroying malaria parasites; its toxicity for mice and similar small animals was relatively low. Clinical trials were initiated by Dr. A. R. D. Adams and Prof. T. H. Davey at the Liverpool School of Tropical Medicine, and its therapeutic activity was shown to be as great for naturally occurring human malaria as it was for infections in the laboratory. Later Dr. Adams was joined by Prof. B. G. Maegraith, who explored its pharmacology in volunteers and the way in which it was absorbed, distributed, and excreted by the body. Supplies of the compound were sent by air to Australia, where its action in preventing the development of malaria following exposure of volunteers to infected mosquitoes was investigated by a team at Cairns working under the direction of Brig. N. Hamilton Fairley. Other studies on various aspects of this compound are in progress in certain military hospitals in Britain, at the London Postgraduate Medical School, and in India.

Altogether the compound has now been given to over 200 patients. Until its use is more widespread any account of its action must be provisional. It is a white powder which does not stain the skin as does mepacrine. The chemical formula is not yet disclosed, but it is relatively simple and differs fundamentally from previously known antimalarials; the manufacture will be easy. When given by mouth the drug is rapidly absorbed. Excretion is somewhat slower, and on repeated twice-daily doses the plateau of the concentration in the plasma is not reached for several days. About a third of the dose is excreted in the urine. The compound is very well tolerated, and up to half a gramme can be given twice daily; the only untoward effect produced to date by larger doses has been signs of gastrointestinal irritation, which might be avoided by subdividing the dose. So far the course of treatment has consisted of twice-daily doses for fourteen days. The smallest dose on this regime which will terminate a clinical attack of benign tertian malaria appears to be 10 to 50 mg., which is only about a fifteenth of the maximum tolerated dose. Unfortunately (as with quinine and mepacrine) relapses of benign tertian often occur, and even with large doses a quarter of the patients have relapsed. The response of malignant tertian malaria resembles that of benign tertian, but (as with other drugs) relapses are less frequent. The effect of the drug on quartan malaria has not yet been studied. If used to protect men against malaria, when exposed to infected mosquitoes, it has been found that 0.1 g. daily prevented all clinical signs of malaria in a small group of volunteers who were bitten by mosquitoes carrying *P. vivax*, although parasites were later demonstrated in a small proportion of the subjects. Apparently this antimalarial action is a suppressive one, exerted upon the trophozoites as soon as they appear in the erythrocytes. The action upon *P. falciparum* is more complete and there is less tendency for a latent infection to be acquired. Gametocytes are injured by the drug, but they are not completely removed from the circulation.

So far as can be seen from the information now available the new drug acts upon the same stages of the malaria parasite as does quinine or mepacrine, but there is a wider

[6] *Lancet*, 1944, **2**, 715, 716, 717.
[7] *Science*, 1944, April 28, p. 351.
[8] *J. Amer. med. Ass.*, 1945, **128**, 175.
[9] *J. biol. Chem.*, 1945, **159**, 725.

range between the minimum effective dose and the maximum tolerated one. The greatest need in malariology at present is a drug which will prevent relapses due to benign tertian malaria. Although not dangerous, these relapses are troublesome. The new compound certainly does not prevent all relapses ; whether it prevents more or fewer than do quinine and mepacrine cannot be told until certain comparative trials now in progress are completed. It should be cheaper to produce than either quinine or mepacrine, and it is free from the drawbacks of the latter, which stains the skin yellow. As a suppressive, to prevent clinical malaria following exposure to infection, it seems to be equal or superior to mepacrine and much superior to quinine. Provided that widespread use reveals no occurrence of dangerous idiosyncrasies, such as agranulocytosis or nervous lesions, it may well displace quinine and mepacrine from general use in malarious areas.

Before congratulating Imperial Chemical Industries and the research workers concerned on this brilliant discovery, a minor criticism must be made against the nomenclature of the substance. "Paludrine" is an excellent euphonious word of obvious significance, but unfortunately it had already been applied to an earlier less successful product (compound M.3349) and as such had appeared in a medical journal.[1] For some time to come this use of the same name for two different substances will cause needless confusion, and the new product will have to live down the less favourable impression which may have been created by the older one. Apart, however, from this minor blemish, all concerned in this discovery deserve admiration and praise—the directors of I.C.I. for their far-sighted support of large-scale chemotherapeutic research, Messrs. Rose, Curd, and Davey for the actual discovery, and the clinical workers named above for their careful exploration of the medical uses of the new remedy which was placed in their hands. The whole story forms a good example of the team work which is necessary for progress under modern conditions. This latest success is valuable not only in itself but also as a proof that British brains and organization, when properly applied, can produce quite as great discoveries as those of Germany or of any other nation. With such encouragement it is to be hoped that this country will no longer delay in giving to research on chemotherapy and tropical medicine the support which our wide Colonial Empire requires.

RADIOTHERAPY FOR VASOMOTOR RHINITIS

Unless the antigen which produces the allergic symptoms of vasomotor rhinitis can be discovered treatment remains unsatisfactory. Even if the antigen or antigens are known, it does not follow either that they can be eliminated easily or that the patient can be desensitized. Local treatment by ionization, by cautery, and by vasoconstrictor drugs all have disadvantages, and the application of radium to the mucous membrane of the nose is an uncertain and dangerous measure. The suggestion that radiotherapy by means of *x* rays might prove effective is therefore worth serious consideration. Axel Renander[2] states that in 1936 experimental irradiation of the nose in a case of pronounced vasomotor rhinitis was followed by complete freedom from symptoms for eight months. Since then he has treated 400 patients in this way, and has recorded a series of 226 cases. He notes that Schmidt[3] reported on a favourable result in a case of hay-fever as long ago as 1915, but very few systematic attempts have been made to ascertain the value of this treatment. Of the 226 patients, 126 were females (56%) and 100 males (44%). Mucous polypus was present in 11 cases (5%)—in 8 men and in 3 women. Of the 226 cases, 133 received one series of treatments only ; of these, 80 did not return, so that the results could not be ascertained. In the remaining 53 cases which were followed up, 41 were free from symptoms, 9 were improved, and in 3 there was no change. Sixty-three cases received two series of treatments. After the first series 38 were free from symptoms or much improved, 19 improved, and 6 showed no change. Of 24 observed after the second treatment, 18 were free from symptoms, 3 improved, and 3 unchanged. In a third group of 22 cases receiving three courses of treatment there was only one in which it had no effect. There were also two other small groups receiving four and five series of treatments respectively. In all these the results were satisfactory. Of the 146 cases followed up, 18 (12%) were insensitive to one or more of the series of treatments ; 8 of these reacted favourably to some of the series of treatments, and in 10 (7%) the treatment had no effect.

The author calculates from a survey of the cases that freedom from symptoms, or at least improvement, may be expected in about 85%. The dosage recommended is 150 *r* repeated three times with an interval of one or two days between the treatments. The sinuses and nasal cavity are irradiated with a filter of 0.5 Cu + 1 mm. Al distance 40 cm., with the eyes and eyebrows protected. Decreasing the dose to 100 *r* or 75 *r* did not produce such good results. The author therefore advises 100 or 150 *r* × 3, after a careful survey of the effects of different doses. Neither anosmia, atrophy, nor ozaena followed in any of the cases, and the sense of smell returned in a few. It appears, therefore, that with such small dosage no damage to the mucous membrane need be feared. It is suggested that the good effect is produced by the action of the *x* rays on the blood vessels of the mucous membrane, rendering them insensitive to the antigens. Should this form of treatment be found effective on further trial, it should be a valuable addition to the management of a troublesome condition hitherto difficult to treat.

ERGOTAMINE FOR MIGRAINE

It is now generally known that ergotamine tartrate is effective in treating typical migraine ; this is defined as a condition in which there are periodic headaches, usually hemicranial, associated with nausea, vomiting, scotomas, and a family history of similar headaches. The use of ergotamine tartrate for this purpose was introduced by Maier[4] in 1926 ; it is usually given by subcutaneous or intramuscular injection in a dose of 0.25 mg. or 0.5 mg., not more than 1.0 mg. being administered in 24 hours. If it is given orally in tablets to be allowed to dissolve under the tongue larger doses must be used, since absorption is less complete, and, after an initial dose of 5.0 mg., 2.0 mg. may be taken hourly to a total of 11.0 mg. On the whole it is preferable to inject ergotamine, as there is less likelihood of untoward effects. Von Storch[5] has reviewed 42 accidents described in the literature, of which 21 were gangrene ; ergotamine is of course one of the active principles in ergot which have led to gangrenous ergotism. Accidents are likely to arise only when ergot-

[1] Das Gupta, B. M., Lowe, J., and Chakravarti, H., *Ind. med. Gaz.*, 1945, **80**, 241.
[2] *Acta otolaryngol.*, 1945, **33**, 44.
[3] *Münch. med. Wschr.*, 1915, **62**, 773.
[4] *Rev. Neurol.*, 1926, **33**, 1104.
[5] *J. Amer. med. Ass.*, 1938, **111**, 293.

amine is used continuously, and no serious complications have been observed in migraine. The best results are obtained when it is administered early in the attack, and when care is taken to give no more than is needed.

The mechanism of the action of ergotamine is still unknown. It causes a rise of blood pressure and an increased cerebral blood-flow, but other agents which have these effects, such as adrenaline or inhalation of CO_2, do not always relieve headache. Graham and Wolff[6] state that the curative effect is exerted in parallel with a reduction in the amplitude of pulsation in the temporal artery, and is to be attributed to a constriction of the cranial vessels.

Recently Horton, Peters, and Blumenthal[7] have tried the effect of a derivative, dihydroergotamine. This is stated to be less toxic than ergotamine and to produce gangrene to a much smaller extent. No experimental details in support of these statements have yet been published, and it is wise to wait for the evidence, since dihydroergotamine is a vasoconstrictor, and like ergotamine produces (in sufficient dose in animals) paralysis of the sympathetic. Nevertheless evidence from 120 patients indicates that this substance is as effective as ergotamine tartrate in the treatment of migraine, 75% of the patients receiving relief. (This is less than the 90% described as getting relief by Lennox and von Storch.[8]) If it is true that dihydroergotamine is less prolonged in action than ergotamine, this property would make it a safer substance to use.

It is interesting to note that in the period of nearly twenty years since ergotamine was used for migraine no one has thought it worth while to investigate the value of ergotoxine. This substance ought to have precisely the same action as ergotamine in migraine, judging from its pharmacological properties.

BIOTIN

Biotin, the most recently discovered member of the vitamin B complex, is a growth factor for many bacteria and moulds and an essential nutrilite for most animals, and possibly man, in whom biotin deficiency has been reported by two groups of observers. Experimental biotin deficiency in animals has been brought on by limiting the biotin intake over long periods; including in the diet raw egg-white, which inactivates the vitamin; and by adding sulphonamide drugs to an adequate diet. The feeding of sulphonamides, particularly sulphaguanidine and succinylsulphathiazole, is one of the most recent methods of inducing vitamin deficiencies in animals. These drugs act by interfering with the bacterial synthesis of vitamins in the intestine, particularly the synthesis of the vitamin B complex and vitamins E and K. Waisman and his colleagues[9] have recently produced chronic biotin deficiency in monkeys by all three methods. This is characterized by a thinning of the fur, a gradual loss of colour of the hair, and in the later stages a heavy, scaly dermatitis covering the whole body but most conspicuous on the face, arms, and legs. The loss of hair is also affected by hormonal influences, as indicated by the slower regrowth of fur in menstruating and adolescent females. Twenty μg. of biotin daily was sufficient to prevent the deficiency symptoms. The biotin deficiency from feeding egg-white was similar to that from adding sulphonamides to the diet. The prothrombin time was not appreciably affected—a finding in contrast to the increased prothrombin time found in rats given sulphaguanidine or succinylsulphathiazole with their food.[10] Biotin deficiency in man, caused by administering large quantities of egg-white, has been reported by Sydenstricker and his co-workers.[11] They observed a greyish pallor of the skin, anaemia, atrophy of the lingual papillae, and increasing dryness of the skin, which progressed to a maculo-squamous dermatitis of the neck, hands, arms, and legs. A decreased excretion of biotin was observed after seven weeks on the egg-white diet.

Experiments have recently been made on the vitamin activity of biotin isomers and analogues. Biotin is a *d*-compound. The *l*- and *dl*- (or racemic) compounds are active only when tested on bacteria in very large quantities,[12] although when assayed on rats maintained on a ration containing egg-white *dl*-biotin is stated to be half as active as biotin.[13] When assayed on chicks *l*- and *dl*-biotin show no activity at all.[14] It is indeed remarkable how slight changes in the chemical constitution, or even the optical activity, of a vitamin can profoundly affect its physiological activity. This is seen not only in the case of biotin but also with other B vitamins. Slight modification of the chemical constitution of pantothenic acid,[15] vitamin B_1,[16] and riboflavin,[17] for example, results in products which are not only devoid of any vitamin-like action, but when fed to animals actually produce deficiency symptoms identical with those produced by lack of the vitamin they resemble chemically.

A JOURNAL OUT OF BONDAGE

Before the war the journal of the Norwegian Medical Association—*Tidsskrift for den Norske Laegeforening*—was a fortnightly publication in a faded brick-red cover of small size but with quite meaty contents. Soon after the German invasion a handful of quisling doctors took it over. Dr. J. H. Berner, its editor for many years, was dismissed and locked up. The colour and format of the journal remained, but its contents became a quaint parody of the past. Its cover was the cover of medical science, but its voice was the voice of Terboven and Quisling. The writers of original articles would no longer write for the *Tidsskrift*. So the decree went forth that no Norwegian doctor could publish his observations elsewhere without the permission of the reigning authorities. Whereat original articles remained locked up at home, awaiting better times. So the captive journal came to consist mainly of abstracts (from German publications, of course) and of editorial notices drafted alternately in truculent tones and come-home-to-mother sobs. Now the old staff of the journal is back in place, and to celebrate its liberation it has changed its colour and format. The upper third of the cover is a greyish-green; the lower two-thirds are white. The format has grown, and it is now a few centimetres larger in both directions than the *British Medical Journal*. The contents are a judicious blend of home and foreign medicine—altogether a very agreeable set-up. We send greetings to an old friend in new guise.

The Central Medical War Committee is informed that Royal Air Force medical officers in age-service groups 19 and 20 will be released during November. These groups have been brought forward from December. Groups 21 and 22 will be released during December, and Group 23 during January, 1946.

Sir John Boyd Orr, M.D., F.R.S., has been elected Director-General of the Food and Agriculture Organization of the United Nations, now in conference at Quebec.

6 *Arch. Neurol. Psychiat.*, Chicago, 1938, **39**, 737.
7 *Proc. Mayo Clin.*, 1945, **20**, 241.
8 *J. Amer. med. Ass.*, 1935, **105**, 169.
9 *J. Nutrit.*, 1945, **29**, 1.
10 Black, S., *et al.*, *J. biol. Chem.*, 1942, **145**, 137.
11 *J. Amer. med. Ass.*, 1942, **118**, 1199.
12 Stokes, J. L., and Gunness, M., *J. biol. Chem.*, 1945, **157**, 121.
13 Emerson, G. A., ibid., 1945, **157**, 127.
14 Ott, W. H., ibid., 1945, **157**, 131.
15 McIlwain, H., and Hawking, F., *Lancet*, 1943, **1**, 449; Barnett, J. W., and Robinson, F. A., *Biochem. J.*, 1942, **36**, 357.
16 Woolley, D. W., and White, A. G. C., *J. biol. Chem.*, 1943, **149**, 285.
17 Woolley, D.W., ibid., 1944, **154**, 31.

TUBERCULOSIS AMONG PRISONERS OF WAR IN GERMANY

BY

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During my four years as a prisoner I worked in a transit camp, a "Krankein-revier" (camp reception station), doing sick parades for the surrounding camps, a general hospital, and the special hospital for cases of tuberculosis. All these hospitals were mixed—i.e., contained prisoners of war of more than one nation—and in all of them I looked after the Tb.s, so that I was able to get some idea of the general picture of tuberculosis among these prisoners in Germany.

General Considerations

The living conditions of P.O.W. workers in Germany varied. They depended on the area of Germany, the type of work, the nationality of the prisoner, and the character of the Kommando Führer. In general the Russians had the worst conditions; all Russian Kommandos that I saw were overcrowded, badly ventilated, and dark, but I was not allowed to see many. The British and Americans, in general, had the best conditions, but even they were usually overcrowded.

Diet also differed: the German rations varied according to the type of work done, and also, to some extent, according to local supplies and nationality (Russians until the summer of 1944 received lower rations than others doing the same type of work). It is always difficult to determine from hearsay how much someone else is eating, so I am giving here only the diets I myself received. Doctors, orderlies, and patients were classed as "Normal-Arbeiters" along with the non-manual workers, and their diet in the various places where I worked was as follows:

1941: Transit camp	Approx. 1,000 cals. p.d. for 6 months
1942–3: General hospital	„ 1,600 „ „ „ 2 years
1944: Special hospital for Tb.s ..	„ 1,800 „ „ „ 1 year
1945: "Revier"	„ 1,300 „ „ „ 5 months

With such low basic diets, Red Cross food parcels became very important. The Americans and British were fortunate in usually having a parcel a week, which added about 1,500 calories a day to their diet. The French until 1944 also received parcels regularly, but Serbs, Poles, and Italians received very little and the Russians nothing at all.

The Germans apparently realized that such conditions were likely to produce a high incidence of tuberculosis, so in 1942 they instituted routine mass radiography. I was told verbally by a German that their reason for doing this was not humanitarian, but their fear that tuberculous P.O.W.s would infect German civilian workers. This view of the matter was supported by the German refusal to *x*-ray officers at Oflag (who had little contact with the civilians) until long after they had *x*-rayed all the working camps in the same area, and also by the lack of treatment and facilities given when the diagnosis had been made. The *x*-raying was carried out efficiently and quickly by mobile units, usually staffed by women. No figures are available as to the proportion of the P.O.W.s who were *x*-rayed. It was probably about 60%; but it is only fair to say that in some areas some P.O.W.s had two mass radiographs in five years. The diagnosing of the films was in general poor. There were also cases—far too many—where the films were correctly diagnosed but no action was taken, and the P.O.W. was left for another year spreading bacilli until he was diagnosed clinically.

Some idea of the poorness of the evaluation of the films can be obtained from the figures from Elsterhorst. During 1944 I analysed 68 consecutive sputum-positive cases admitted into Elsterhorst under my care. Of these, 19 had been diagnosed by means of radiography; the remaining 49 were diagnosed by clinical means. Of the latter, 13 had had routine skiagrams and been "missed," or were found positive but no action had been taken. It was very easy to check up on these cases, as the small skiagrams arrived with the patients with the diagnosis attached—*x*-ray positive or negative. It is important that the double danger of these "missed" cases should be realized. It is psychologically very difficult for an M.O. to make a diagnosis of tuberculosis when he knows that the patient has recently been radiographed and been declared negative. Another danger, due to the lack of explanation given by the Germans, arising from the mass radiography was the "Tb. neurosis" which spread rapidly among the prisoners.

The "Revier" and the Sick Parades

When doing such jobs the P.O.W. medical officer was responsible for the sick parades for all the surrounding working Kommandos and had a 50-bed hospital at his disposal. I did this work at Wittenberg for the last five months of the war, and it is on that area that most of these remarks are based. Some idea of the incidence of tuberculosis is given by the following figures for the French and Russian P.O.W.s in this area.

Population.—There were in 1943 1,000 French and 1,200 Russians. The number of French remained remarkably constant; the number of Russians fell to 1,000 in 1945. All these P.O.W.s were mass *x*-rayed in 1942 and the early part of 1943. All those who were found positive were removed to hospital. The figures thus give the incidence of pulmonary tuberculosis in a population from which practically all *x*-ray-positive men had been removed by the spring of 1943.

Procedure.—Suspects were discovered by the British M.O.s on sick parades, and a portion of these suspects were screened by a German M.O., who decided, rather arbitrarily, whether they were to be considered as cases of active tuberculosis or not. Only those accepted by the German M.O. are included in these figures, which can therefore be considered as minimal.

Figures.—A double check was possible through the "screening" reports and the admission and discharge book. The figures were also checked by my Dutch secretary. Those for 1945 are given for the first three months only.

	French (1,200)	Russian (1,200–1,000)	British (250)
1943, July–Dec.	0	6	0
1944, Jan.–June	1	14	0
„ July–Dec.	1	31	0
1945, Jan.–April (3 months) ..	4	11	0

Comment on the high incidence of tuberculosis among the Russians is almost superfluous. Reference has already been made to the conditions under which they lived and the diet they received. In Wittenberg it was striking that there were scarcely any big Russians left in 1945: lower metabolic demands had a very definite survival value. The raised incidence among the French in 1945 was doubtless a reflection of the absence of parcels from France after 1944.

Another interesting feature of Wittenberg medical life in 1945 was the appearance of 350 Danish policemen. These men, who were all of outstandingly good physique, had been made prisoners of war in Sept., 1944. They spent three months in a concentration camp and then came to a working party in Wittenberg. In the concentration camp the living and working conditions were fearful, but their diet was adequate, as they got excellent food parcels from Denmark weekly. They stayed only two months at Wittenberg, but I diagnosed seven cases of open active tuberculosis and four of pleurisy with effusion. In this instance it seemed that living conditions, other than diet, were mainly responsible.

On these sick parades the early diagnosis of tuberculosis presented quite unusual difficulties. They lasted from 8 a.m. until frequently to 2 p.m., and after 11 o'clock my efficiency deteriorated, particularly as we were at the time without parcels and on the 1,200-cal. German diet. With me latterly was a German sergeant, trying to get me on a sabotage charge, so the atmosphere was not restful!

Then there were the languages—Russian, Serbian, French, Dutch, Indian, Danish, Slovakian, Italian, Polish—to say nothing of the German I had to talk to the sergeant. I learnt to take a decent tuberculosis history in Russian, French, Serbian, Dutch, Italian, and German, and then to my dismay found I was no further on. All the patients were tired and wanted a rest; all of them wanted to do as little work as possible for Nazi Germany. I was a new doctor, and it was unreasonable to expect them to trust me at once; so I found that all Serbs and Indians said they suffered from haemoptysis, and all Serbs gave gruesome family histories. All Frenchmen seemed to have had pleurisies (they muddled up "pleurite" and "pleurisie").

Serbs and Indians all gave good histories of night sweats, and nearly everyone coughed and really had lost weight. The Russians were interesting; they rarely admitted tuberculous symptoms. I discovered the reason for this later; they were terrified of being sent to the special hospital for Russian cases because they thought everyone died there, and, so far as I could make out, they were quite right. The advanced Russian cases, while waiting at Wittenberg "Revier" for transport to "Zeitheim," actually escaped to avoid going there.

When the histories proved so fallacious, I tried doing a larger number of B.S.R.s. Here too I found new difficulties. The high incidence of skin disease and slight hypoproteinaemic oedema made the B.S.R. more difficult than usual to interpret.

Having collected one's suspects, the next step was to get them *x*-rayed or screened. In many areas this was very difficult indeed, the Germans being most obstructive. At Wittenberg I was lucky in that I finally persuaded the German M.O. to let me screen 10 to 12 cases with him each week. In some other areas a positive sputum in addition was demanded before cases could be sent to hospital, and the Germans tended to argue that one negative sputum was proof that the case was inactive. This was particularly dangerous, as the German laboratories were very slack about P.O.W. sputa. There were also other difficulties. I once sent three sputa from one case to three different laboratories. Each one was said to have been destroyed by bombing. German M.O.s claimed that if they heard nothing abnormal clinically there could be nothing wrong with the patient.

The General Hospital (Reserv Lazarett)

My experience of this comes from looking after Serbian, French, and British tuberculosis cases at a Reserv Lazarett for two years. Here the problems of the early diagnosis of tuberculosis were slightly easier, as one could do one's own sputa and there was more leisure for multilingual activities; but the German doctor would never accept the sputum results done by prisoners of war—they had to be sent to a German laboratory; and I had for a time to take histories in Serbian and write them in German!

As regards screening, I had the same arrangements at Wittenberg. I screened about 6 suspects a week with a German civilian doctor—in this case a woman. At one period a German M.O. made a written protest against my being allowed in a dark-room with a German Frau—it was apparently a risk to the purity of German blood (!); but after a short interruption the screenings continued.

The great problem at the Reserv Lazarett was that of treatment. This should never have been a problem at all, as the cases should have been evacuated to the special hospitals; but in practice they stayed three to six months. There were many reasons for this: (*a*) Owing to lack of repatriation the special hospitals got overfilled. (*b*) The Germans made absurd transport rules. One was that you must have six of one nationality before you could have a transport. We then had relatively few British, and by the time we had five cases of tuberculosis the old ones started dying. (*c*) Sheer obstructionism: on many occasions when the Chef-arzt stated that he had asked for transport we found through our spy in the office that he had not. (*d*) In some general hospitals there was a regrettable lack of information about even the facilities at the special hospital, and cases were often kept for over a year, and cases of A.P.T. with adhesions were abandoned before being sent there. This lack of information was entirely due to the inefficiency of the Germans.

The basic diet, as already stated, was about 1,600 calories in these hospitals, and of this the tuberculosis patients found a large proportion uneatable. Frequent requests were made for extra food, but these were always refused until early in 1943, when a little milk was allowed, and later occasional eggs and white bread; but the available diet from the Germans was never more than 1,900 calories a day. The British patients did not suffer, as a satisfactory diet could always be made up from Red Cross food parcels and invalid comfort parcels; but the problem was very difficult for a British doctor looking after tuberculous Serb and French patients. (There were no Russians in this particular hospital.) We were unable to get any instructions from London, and there was unfortunately some feeling against giving British food to other nationalities. I think, however, that all British M.O.s did give as much food as could be spared; I certainly did, and the results were well worth it.

Active treatment was very difficult there—the German doctor had never heard of it. Finally in 1943 I got an A.P.T. machine, but was then forbidden to use it, because the German doctor believed that A.P.T. cases could not travel in trains and thus could not be evacuated. The patients at Hospital 1251 did get complete rest, but often under difficulty. One reason for this was the shortage of orderlies. At one stage we had seven orderlies for 120 patients, of whom 20 were cases of tuberculosis. These orderlies had to fetch food and run the laboratory in addition to their ordinary day and night duty. They, too, were afraid of treating the disease, and some became very neurotic.

When working at Elsterhorst one was able to get some idea of the amount of rest tuberculous patients were obtaining at the various other general hospitals in Germany. On the whole the information was very depressing. Only a few of the open active cases were at complete rest; some had even been encouraged to do physical jerks, many had been for walks, many did their own cooking. There seemed unfortunately to be a widespread belief that if a patient was afebrile he could get up and walk, however big his cavity. That was found not only among German but also among Allied and British doctors.

It is worth while mentioning the cemetery at Hospital 1251. The P.O.W. section there served both a surgical and a medical hospital, but 80% of the graves were filled with victims of tuberculosis. This, I think, gives some idea of the prevalence of the disease in imprisonment.

The type of transport between general hospitals and the special tuberculosis hospitals was very bad. Originally patients travelled in unheated cattle-trucks without even straw and orderlies. After persistent complaint we evolved step by step—getting first straw, then orderlies, then heat, and finally reserved third-class compartments. Later we degenerated again. I remember in 1944 some tuberculous Canadians arriving at Elsterhorst. They included a case of tuberculous meningitis, a spontaneous A.P.T. with empyema, and a case of tuberculosis with diabetes; they had travelled three days in an unheated cattle-truck, with no orderlies, little food, and no insulin. In spite of innumerable complaints travelling facilities got worse and worse as the end of the war approached.

Special Hospitals

1. "Königswartha" was originally a hospital for tuberculous French P.O.W.s. The first English cases arrived there after the fiasco of the Rouen evacuation. Originally a blind asylum, it had limited accommodation, poor cooking facilities, pleasant gardens, and bad sanitation. At that time the repatriation prospects were *nil*, and inevitably the hospital developed into a pleasant place to die in rather than a serious hospital, although much good work was done there.

2. Elsterhorst became the chief hospital for tuberculosis in 1943 after numerous complaints from the British about the overcrowding at Königswartha. This was a modern German standard hutted hospital. The cooking facilities, the sanitation, and the wards were good, but unfortunately it was situated in the middle of a sandy plain, and there was a severe water shortage and no shade in summer. The hospital was six kilometres from the nearest station, and inevitably a large number of advanced bilateral cases were forced to walk that distance carrying their kit. There was, too, the inevitable wire round the hospital. Security measures were not too severe until 1944, when a stringent curfew at 9 p.m. was introduced, and doctors and orderlies were not allowed to move from one barrack to another after that time without a guard. As it took five or ten minutes to get a guard this made emergencies at night very dangerous. Searches also started at this time; it was an odd sight to see soldiers of the Third Reich search men with advanced tuberculosis in bed and remove excess food—in case they escaped! Actually the soldiers were ashamed; they did the work gently, and it cleaned the lockers. There was another odd sight when a big convoy of tuberculous patients arrived from Lamsdorf. The truck drove into the hospital and was then surrounded by soldiers with fixed bayonets to prevent doctors getting near until the men had been searched. I was finally thrown out of the hospital when I stated that it was

"ein Skandal im Lande von Robert Koch" when the Chefarzt ordered me to put a febrile bilateral A.P.T. case with fluid on to an open truck with snow falling and the temperature well below zero.

Dietetically the Germans were not over-generous; they provided about 2,000 calories a day, of which ill patients could manage to eat only part. However, with the help of food parcels, invalid comfort parcels, and a black market, the patients were probably the best fed in Europe. They suffered only from a lack of fresh vegetables and a somewhat monotonous menu. It was very striking that, there in the heart of Germany in the middle of the war, three-fifths of the food eaten should come from the Allies.

All the usual methods of collapse therapy were employed in the hospital, except Monaldi's, and on the whole the patients rested well. It is difficult to present any picture of the type of treatment given in a hospital without going into great detail, but the following figures from a series of consecutive positive cases in 1944, when the hospital was at its best, may help:

Positive cases with at least 3 months' stay in hospital	44
No. of A.P.T.s attempted	39
„ „ successful	31

Of the A.P.T.s 2 were made bilateral and 5 were abandoned. Of the 13 failed or abandoned A.P.T. cases 7 later had a phrenic avulsion and one in addition a rather unsuccessful pneumoperitoneum Of the successful A.P.T.s which were not abandoned (26) 20 had thoracoscopies with or without adhesion-section. Of all these, one died at Elsterhorst and 24 became negative before leaving, after an average of 3.3 months in hospital—i.e., 54.4%. The latter figure should not be taken too seriously—the total is too small and our facilities for detecting bacilli were not very advanced; but all the cases which I have so far checked up on in England have remained negative, and several more have become negative.

The hospital was originally completely international; there were Australians, New Zealanders, Canadians, South Africans, Cypriots, Indians, Americans, French, Poles, Serbs, Dutch, Norwegians, Spaniards, Italians, negroes, to say nothing of the English, the Welsh, the Irish, the Scots, and the Channel Islanders, among the patients; and the Russians helped with the work of the hospital. On the whole they lived together in peace—chiefly, perhaps, because they all received British Red Cross parcels!

It would be wrong to mention Elsterhorst without naming Lieut.-Col. Le Souef, A.A.M.C., and Major Wallis, R.A.M.C. Everyone at Elsterhorst owes them a great debt of gratitude, and I am sure the vast majority share my admiration and affection for them both.

Nova et Vetera

THE HISTORY OF SYPHILIS

In the month of February, 1495, Charles VIII of France arrived with his mercenaries before the city of Naples. The city fell almost without opposition, and there followed in the armies of both sides, and in civilians, an outbreak of a virulent disease which had not been long known in Europe. The results of this mixing of mercenaries and civilians were tragic, and when Charles disbanded his army at Lyons seven months later the disease was already causing many deaths in addition to horrible features and painful symptoms. Ruy Dias de Isla called it the *Morbo Serpentino*—hideous, dangerous, and terrible as the serpent itself. Throughout much of the following century the disease was epidemic, and presented all the characters of any serious epidemic disease which attacks an unprotected population. Thereafter the people of Europe developed some degree of immunity, so that the disease now became more remarkable for its chronic manifestations. In 1530 Fracastorius, the greatest of the early epidemiologists and the wielder of a Latin measure almost as stately as that moulded by the swan of Mantua, wrote a poem in which he named the disease "syphilis." Nearly two centuries later the name found its way into medical writings. In the four and a half centuries since it first attracted attention it has been the subject of a vast literature. There have been many histories of syphilis, but there is even now no agreement on certain moot points. In a recent Vicary Lecture at the Royal College of Surgeons Mr. J. Johnston Abraham[1] tackled the subject again, and his contribution will be read with great interest even by those who are familiar with the subject.

There will always be those who regard the first origin of syphilis in Europe as a purely academic question. Mr. Johnston Abraham does not commit himself on this point, but it is perhaps not difficult to see where his sympathies lie. Although he says that syphilis was *apparently* an unknown disease in Europe in 1495, in his discussion of the Columban origin he cites writers such as de Isla, Fernandez de Oviedo, and las Casas, who were impressed by the violence of the outbreak—unlike anything they had previously known. There is an important school which believes that syphilis had been known in Europe since the earliest times. Sudhoff, for example, considered that mercury was being used from at least the twelfth century for certain skin conditions which were probably syphilitic. The matter will probably never be settled. Apart from the dramatic events of the fifteenth and sixteenth centuries, the author concentrates in this lecture mainly on methods of treatment and on the monist and dualist theories of the disease. The condition which we now know as gonorrhoea had been recognized for centuries. John Arderne mentioned "chaude pisse," as it was then termed. Paracelsus lent his great weight to the theory—advanced shortly before his time—that syphilis and gonorrhoea were one and the same disease. The unfortunate experiment which John Hunter performed on himself in 1767 seemed to prove that the monists were right. Although Benjamin Bell published evidence to the contrary in 1793, and Richard Carmichael taught in 1814 that they were different diseases, it was not until the publication of the painstaking experiments of Ricord in 1838 that the dualist theory became widely accepted. There is evidence—for example, the statement of Gaspar Torella—that mercury was used in the treatment of syphilis before the end of the fifteenth century. As a result partly of the unscrupulous action of quacks the orthodox profession was forced to give larger and larger doses, until a course of mercurials was an abominable ordeal for the unfortunate patient. China root and sarsaparilla enjoyed a brief hour on the stage, but guaiac, first introduced in 1517, had a longer run. Wallace of Dublin used potassium iodide in the eighteen-thirties, and Auzias-Turenne in 1850 recommended inoculation of the patient with pus from a soft sore. He continued to advocate his method of "syphilization" for a period of twenty-eight years! The modern armamentarium, based on the work of Schaudinn and Ehrlich, is outside the scope of the lecture.

There are few diseases whose history is so difficult to write concisely and fairly as that of syphilis. Mr. Johnston Abraham has succeeded in a difficult task in this admirable and well-illustrated lecture. Many will also be grateful to him for giving the true derivation of the term "Lock Hospital"—from the French *les loques* (rags), with which the lazars wiped their sores before they entered the lazar houses in the days when these institutions, used less and less for their original purpose, were turned over to the victims of syphilis, the new epidemic disease.

E. A. U.

VARRIER-JONES MEMORIAL LECTURE

As a memorial to the late Sir Pendrill Varrier-Jones, the founder and first medical director of Papworth Village Settlement for the Tuberculous, near Cambridge, an annual memorial lecture has been instituted. The first was given by Wing Commander, now Air Commodore, R. R. Trail, who has succeeded Sir Pendrill as medical director of Papworth and its associated centre at Enham, and the second by Sir Arthur MacNalty, who was Chief Medical Officer of the Ministry of Health from 1935 to 1941. The third lecture of the series is to be given in London at the Hospital for Sick Children, Great Ormond Street, on Wednesday next, Nov. 14, by Dr. Alan Moncrieff. His subject is "Tuberculosis in Childhood," and in it he will make special reference to the exhaustive survey of the children of Papworth, prepared from Papworth records extending over more than 25 years by Dr. E. M. Brieger. This survey, published under the title *The Papworth Families*, establishes the success of Papworth in protecting Papworth-born children against tuberculosis, and demonstrates anew that the children of the tuberculous need not contract the disease and indeed will not do so if the family's domestic conditions permit medical advice to be carried out.

[1] *Brit. J. Surg.*, 1944, **32**, 225.

BRITISH ORTHOPAEDIC ASSOCIATION
ANNUAL DINNER

The British Orthopaedic Association held its Annual Meeting Dinner at Lincoln's Inn on Oct. 26, when some 200 members and guests were present. Mr. St. J. D. Buxton presided. Proposing the health of the B.O.A., Dr. Charles Hill, Secretary of the B.M.A., referred to the contributions which the Association had made in recent years to the profession's planning of medical services and, in particular, of accident services. He added that the B.M.A.'s Report on Fractures was the starting-point of much progress. Referring to present problems and to the new situation with which the profession was confronted, he said that the profession had little to fear provided that in its proposals it adhered to what was in the public interest. The unity of the profession in all its branches was essential at the present time.

In response, Mr. Buxton said that it was in 1918–19 that, thanks largely to Sir Robert Jones, the position of orthopaedic surgery in relation to the Services had been well established. By 1939 much had been forgotten, and orthopaedic work had to be organized by individuals. It was, however, true that the education of young surgeons and would-be surgeons in orthopaedics had developed, and should be developed further, in co-operation with other surgical specialties. Prof. H. J. Seddon gave an entertaining account of the guests, among whom were Lord Moran, Sir Alfred Webb-Johnson, and Sir Wilson Jameson. Sir Alfred, he said, was now breaking down the barriers between specialist and general surgeons, and there was now no antagonism "between the great realm of orthopaedic surgery and the little rump known as general surgery." Replying for the guests, Sir Wilson Jameson, Chief Medical Officer to the Ministry of Health, spoke of the need for interchange between different departments of medical work, and referred to the way in which orthopaedic surgeons had grown accustomed to co-operation with central and local government. He hoped that this example would be widely followed, so that the National Health Service could be built up in the best possible way. The President's health was drunk with musical honours on the proposal of Mr. George Perkins, President-Elect.

HEBERDEN SOCIETY

At the dinner of the Heberden Society last week, Dr. C. W. Buckley, in his presidential address, said that the Society was now entering a new chapter in its history—a chapter which he hoped would be associated with important advances in the study of the common diseases grouped under the term "rheumatic." "They have appeared frequently to be regarded as unworthy of the attention of professors of medicine interested mainly in higher things. . . ." Among men whose names were associated with rheumatism were the Heberdens, father and son, and the Garrods, father and son again. To Archibald Garrod belonged much of the credit for advancing the study of chronic rheumatism. The appointment by the B.M.A. of a committee in 1931 to report on chronic rheumatic disease was an important indication of awakening interest. Three years after this the Royal College of Physicians appointed a committee with a similar object, and the publication of its *Reports* was a forerunner of the *Annals of the Rheumatic Diseases* (now published quarterly by the B.M.A.). Another landmark was the formation of the Empire Rheumatism Council, which owed its inception largely to the vision and energy of Lord Horder. In addition to its propaganda work, the Council had done much to direct and subsidize research. When the Empire Rheumatism Council was being formed the medical staff of the Red Cross Clinic formed with others a society for the discussion of the clinical and pathological aspects of chronic rheumatic disease, and adopted the name of Heberden. Notable among the researches carried out by members of this society had been the work of Copeman and his colleagues on fibrositis. The revival of the Heberden Society had been marked by its affiliation with the Empire Rheumatism Council, and its proceedings would be published in the *Annals of the Rheumatic Diseases*.

General practice, Dr. Buckley went on to say, provided an unrivalled field for clinical observation on hereditary, family, social, occupational, climatic, and meteorological influences, and he hoped that the Society would encourage practitioners to study these problems. He questioned whether any group of diseases had suffered more from methods of treatment advised without any consideration of the underlying pathology. There were few cases in which properly directed physiotherapy was not a most essential part of the treatment of chronic rheumatism; but a wide acquaintance with physical methods did not make a specialist in rheumatic diseases. The primary object of the Heberden Society was the study of rheumatic diseases from the standpoint of clinical medicine and pathology. At the moment the number of members was limited to 100.

After the presidential address, the toast of the guests was proposed by Brigadier F. D. Howitt, who extended a special welcome to Colonel C. W. B. Littlejohn and Colonel J. H. Anderson of the Australian Army Medical Corps, to Sir Frank Fox, secretary of the Empire Rheumatism Council, and to Mr. Tom Williams, the Minister of Agriculture and Fisheries. Mr. Tom Williams and Colonel Littlejohn then replied on behalf of the guests.

RESEARCH ON AGEING

In 1939 a Club for Research on Ageing was established. This actually represents a team of medical and scientific research workers for study and research on ageing, and is not a society in the ordinary sense of the word. There are branches of the club in Great Britain, the United States of America, France, Argentina, and also members in Denmark and Switzerland. A branch in Sweden is in process of formation.

The governing bodies of the branches of the club consist of the following members of the British branch—Prof. G. R. Cameron, Prof. E. C. Dodds, F.R.S., Prof. Sir Robert Robinson, F.R.S. (chairman), Lady Robinson (hon. treasurer), Prof. M. J. Stewart, Dr. V. Korenchevsky (hon. secretary); of the branch in U.S.A.—Prof. W. de B. MacNider (chairman); Dr. E. J. Stieglitz (secretary), and of the executive committee—Prof. E. V. Cowdry (chairman), Prof. A. B. Hastings, and Prof. H. S. Simms; of the group in Argentina—Prof. B. H. Houssay (chairman) and Dr. E. E. Krapf (hon. secretary); of the French branch—Prof. R. Courrier (hon. secretary).

The number of members is limited; there are only 75 at present, and only those scientific and medical research workers are invited to join the club who are engaged in or direct research on ageing, or promise to do so, and who belong to qualified research laboratories or hospitals. Unfortunately the war has badly handicapped the activities of the club, especially in Great Britain and Europe.

Lord Nuffield, and later the Nuffield Foundation, generously supported a scheme of clinical research work carried out in co-operation with the London County Council at one of the Council's hospitals (Tooting Bec Hospital). The results of these clinical trials are now being examined. At the end of 1944 Lord Nuffield also made a donation of £3,000 in order to establish an experimental laboratory for research on ageing, the Gerontological Research Unit, at present housed at the Departments of Zoology and Physiology of Oxford University (head of the unit—Dr. V. Korenchevsky). Thus the foresight and generosity of Lord Nuffield have made actual research work possible in this country.

In the U.S.A. the branch of the club has had annual conferences, at which the schemes of research were discussed and planned; a museum of senile tissues has been established; clinical research on the effects of vitamins on old persons was started at the Elgin Hospital; and finally a *Journal of Gerontology* has been started. For the latter purpose a special "Gerontological Society Inc." was formed, consisting of the members of the American branch of the club. The editor-in-chief of the journal is Dr. Robert A. Moore, professor of pathology of Washington University. The committee on publications consists of Professors Roy G. Hoskins, M.D. (chairman), and W. de B. MacNider, M.D., and Drs. P. Bronson, L. K. Frank, and E. J. Stieglitz. The correspondent for Great Britain is Dr. V. Korenchevsky.

The journal will be published quarterly, the first number appearing at the beginning of 1946. Besides original papers, it will contain reviews and abstracts on gerontology. It will also contain a special section, "Browsing through the Ages," devoted to a scholarly treatment of some topic from the field of art or literature related to ageing and the aged. The address of the *Journal of Gerontology* is 507, South Euclid, St. Louis, U.S.A.

D. Solomons (*J. roy. nav. med. Serv.*, 1941, **31**, 178) in the course of a year saw 19 cases of infective mononucleosis, in 8 of which meningism was the outstanding feature; in 4 a rise in the pressure of the cerebrospinal fluid was the only abnormality. Nothing abnormal was found in the fluid of any of the other cases.

MEDICAL PRISONERS IN SINGAPORE

The following letter from Dr. R. E. Anderson, honorary secretary of the Malaya Branch, has just been received by the Secretary of the B.M.A., and in view of the interest and especially of the concern we all feel in the fate of our colleagues in the Far East we publish it in full.

c/o The Bank of New South Wales,
Melbourne, Victoria,
Australia.
October 18, 1945.

Dear Dr. Hill,

I am writing to you following on my arrival in Australia after release from internment in Singapore to enclose lists of the medical profession who were civilian internees or military prisoners in Singapore. I have marked with an "M" all those who were on our list as members of the Malaya Branch at the time of the fall of Singapore.

I regret to inform you that four civilian doctors died during their internment: Dr. C. A. Stanley, Dr. J. H. Bowyer, Dr. E. S. Lawrie, and Dr. H. J. Lawson; all of them were members of the Malaya Branch. Dr. Norris was killed while working at Kandang Kerbau Hospital shortly before the capitulation; he was also a member. As far as I know none of the Malayan doctors who were in the prisoner-of-war camps died, but it is regretted that Dr. D. F. Irvine and Dr. W. R. Logan, both members, and Dr. Donaldson (of the Malacca Medical Board) were killed in the fighting which preceded capitulation. Drs. Stanley and Bowyer died as a direct result of the cruelties of the Japanese Military Police, the former dying while in their hands and the latter was returned to us *in extremis*. Of those whose names appear on the lists a few are invalids and received all care and attention on release, but I am happy to say that the majority of us are reasonably well and have all been repatriated.

As you have doubtless heard, conditions in Japanese prisoner-of-war and internment camps left much to be desired; overcrowding was the rule and semi-starvation, especially in the past year, was common. I am not in a position to give information regarding the prisoner-of-war camps, and what follows applies to civilian internees. We were housed for the first two years of our internment in the Changi Prison, Singapore, over 3,000 persons being housed in a criminal prison designed for 600 delinquents. For the last year and a half we were housed in the Sime Road Camp, former R.A.F. headquarters which had been burnt out and rebuilt. While conditions on the whole were better in the second camp the reduction of rations and lack of suitable food and drugs brought many to the verge of grave deficiency disease. All admit that had it not been for the efforts of our profession and the expedients adopted such diseases would have taken a heavier toll, and excellent work was done by our medical staff, despite the lack of essential foods and drugs, to maintain a reasonable standard of health.

The chief medical officer of the camp was first of all Dr. J. H. Bowyer (since dead), and then Dr. J. W. Winchester, our president-elect for 1942. The chief health officer was Dr. D. W. G. Faris, and both these men were assisted by medical and health officers from the Malayan Medical Service and from private practice. Mention must also be made of a committee to which all nutritional questions were referred; it consisted of Dr. G. V. Allen (Editor of the *Journal*), Dr. W. R. Duff, and Dr. R. G. Scott MacGregor. These men by their sound advice and unerring judgment have been instrumental in saving much serious illness. But to mention all by name would be invidious; there were too many of us to permit all of us to function as doctors, and many of us (including myself) played the part of the ordinary internee and worked on general fatigues, which included wood-cutting, gardening, kitchen work, and latterly work of a military nature forced upon us by the Japanese. But I must not omit to mention the good work done by the women doctors, Mesdames Hopkins, Smallwood, Worth, Robinson, and Misses Williams and Elliot, who, with the assistance of sisters and nurses from the Malayan nursing service, ran the hospital for female internees with conspicuous success.

The conditions and medical arrangements were of the worst: for some time we were permitted to send patients to a civil hospital for treatment, but latterly patients were not allowed to leave the camp. The supplies of drugs and dressings were extremely limited and the Japanese medical officer of negative assistance; in fact the impression gained from the few Japanese medical officers we saw was that they were not interested. I believe Dr. Ando, a locally trained Japanese, did what he could, as might be expected from a member of our Association. The supplies we received from the Red Cross were a godsend, though much of the better stuff did not reach us. I am not in a position here to discuss our hospital records, but I may mention that apart from dysentery, malaria, and tropical typhus, we were singularly free from communicable disease. Deficiency diseases and tropical ulcers were our greatest concern.

We were unfortunately unable after the first year or so to hold professional meetings, as these were prohibited by the Japanese along with all other meetings, although when such were allowed we held them under the auspices of the Association. Regarding the Association, I had no time to write you before capitulation; as you can imagine my hands were too full. I am afraid that my records are lost, and while I made an effort to send them to Singapore (you will remember my office was in Ipoh) they did not arrive. Until I am able to make further inquiries I must presume them to be lost. After the Japanese surrender I was able to contact Dr. Lee Choo Neo, our Branch Treasurer, and she was able to tell me that some of her records have been saved, since she was not interned. She believes that what cash assets we had will be safe, as the bulk of them were held by the Post Office Savings Bank.

As to the future of the Branch, which at the moment is in suspense, I can only make a rough forecast. At present Malaya is being run by a military administration and the medical men in this administration are all in uniform. It will, therefore, be possible for them to be military members. But our many Asiatic members are still not catered for, and I can see little hope for them until Malaya returns to more normal conditions. When, however, such conditions prevail and the Malayan Medical Service takes over again from the military administration and private practitioners return to their practices I am confident that the Malaya Branch will resume its activities with renewed vigour. I am concerned, however, about our many Asiatic members, whose names will be found in the supplement to the *Journal of the Malaya Branch* of the British Medical Association for June, 1941. They are anxious to receive journals, I know. Perhaps if you communicated with our treasurer, Dr. Lee Choo Neo, at the Lee Dispensary, Bras Basah Road, Singapore, she might be able to contact those who wish to continue membership, and until further arrangements can be made keep you in touch with them. I am unlikely to return to Malaya for at least six months, if not longer, so I regret that until I do I can be of little assistance. While in Australia my address will be c/o The Bank of New South Wales, Melbourne, and I will keep you informed of any change.

Our president-elect, Dr. J. W. Winchester, is at present in Australia; when I write you again I shall give you his address. Dr. W. H. Brodie (Municipal Health Officer, Penang) left Malaya on leave before hostilities began, and I believe he went to England. He was our president, and I am sure will have contacted you if it were possible. You will doubtless have visits or correspondence from many of our members who are now in England or on their way there, and if it is possible I would be glad to have news of them, though I realize that communications with Australia are slow and erratic. I will probably be in England myself in the spring of 1946, when I hope to call on you. In the meantime, I have seen Dr. Dickson, medical secretary of the Victorian Branch, who has offered me the hospitality of his branch and has kindly offered me the service of his clerical staff for any correspondence with you.

In closing I would like to say that it was with great regret that I heard on my release of the death of Dr. G. C. Anderson, with whom I had corresponded in the past. You naturally have my permission to make public all or any part of this letter in the *Journal* should you consider it to be of general interest.

Yours sincerely,
R. E. ANDERSON.

MEDICAL CIVILIANS INTERNED IN SINGAPORE

Malayan Medical Service

M	Allen, G. V.	M	Faris, D. W. G.	M	McMahon, J. E.
M	Anderson, R. E.	M	Field, J. W.		McNab, R. A.
	Baeza, J. I.	M	Fisher, O. E.	M	McSwan, D. M.
	Beattie, D. A.	M	Gray, J. C. P.	M	Mekie, D. E. C.
M	Blackmore, W. G.	M	Gross, R. D.		Molesworth, B. D.
	Boyd, C. J.		Grove-White, R. J.	M	Monro, J. K.
M	Braine, G. I. H.	M	Hardy, E. A.		Nevin, H. M.
	Bynoe, M. L.	M	Holmes, W. E.	M	Pallister, R. A.
M	Calderwood, R.	M	Home, B. F.		Poynton, J. O.
M	Cameron, A. A.	M	Hopkin, D. A. B.	M	Puleston-Jones, W.
M	Cameron, J. A. P.	M	Hopkins, H. O.	M	Ryrie, G. A.
M	Campbell, S. J.	M	Hudson, J. H.	M	Scrimgeour, H.
M	Chitty, E. C.		Jaques, F. V.	M	Shelley, R.
	Cocheme, M. A. X.	M	Johns, B. M.		Shield, A. L.
M	Currid, P. G.		Johnson, R. S.	M	Struthers, E. A.
M	Day, L. F.		Jones, G. E.	M	Swift, S. H.
M	Diamond, W. C. E.		Landor, J. V.		Tyson, W. J.
M	Dickson, I. T.	M	Lowis, R. M. B.	M	Wakefield, T.
M	Dive, H. R.	M	Lowson, J. M. A.	M	Wallace, H. B.
	Divine, T. A.	M	Lowther, A. H.	M	Williams, C. H.
M	English, J. S.	M	McBeth, R. I.	M	Williamson, H. D.
M	Evans, L. W.	M	MacCarthy, C. T.		Wilson, C. S.
M	Evans, T.	M	McGladdery, H. M.	M	Winchester, J. W.
M	Evans, W. G.	M	MacGregor, R. B.	M	Young, W.
M	Eveson, S. W.		MacGregor, R. G. S.		

Private Practitioners

M	Ainsley, W.	M	Elder, E. A.	M	McIlvean, D. D.
M	Allan, H.		Engel, C.	M	McIntyre, F.
	Ansley-Young, W.	M	Geale, W. J.	M	Mackay, A. F.
M	Bain, M. C.		Gibson-Hill, C. A.	M	Mason, J. B.
M	Canton, N. A.	M	Gilmour, G. C. B.		Moudgill,
M	Carlile, J. P.	M	Gunstensen, E.		Sclater, J. M.
M	Dixon, W. J.	M	Heron, J.		Slot, S. A.
M	Duff, W. R.	CM	Hodgkin, F. P.	M	Smith, H.
M	Dugdale, J. N.		Klemperer, H.	M	Tweedie, D. R.
M	Duncan, W. J.		McGinley,	M	Watson, W. H.
M	Egan, J.				

Women's Camp

M	Miss P. R. Elliot	M	Mrs. I. A. M. Smallwood (M.M.S.)
M	Mrs. Hopkins (M.M.S.)	M	Miss C. D. Williams (M.M.S.)
	Mrs. Robinson (M.M.S.)	M	Miss H. I. Worth (M.M.S.)

MILITARY MEDICAL PRISONERS NORMALLY DOMICILED IN MALAYA

	Name	Last Known Location
M	Lieut.-Col. J. Coutts-Milne	Changi
	,, Hutchison	,,
M	,, J. H. Strahan	,,
M	,, G. F. West	Palembang
M	Major R. C. Burgess	Changi
M	,, R. Crawford	Thailand
M	,, A. L. Dunlop	,,
M	,, E. Egan	Saigon
	,, Garlick	Changi
M	,, A. T. H. Marsden	Thailand
M	,, H. R. Morrison	Changi
	,, McGarry	Thailand
	,, Reid	Changi
M	,, P. E. F. Reutley	Singapore
	,, Webster	Changi
M	,, E. D. B. Wolfe	Kranji
	Capt. Donaldson (M.A.M.B.) ..	Believed killed
M	,, E. J. Emery	Changi
M	,, R. Hall (Johore)	Saigon
M	,, R. S. Hardie	Thailand
M	,, D. Irvine	Believed killed, Alexandra Hospital
	,, Jennings (K.L.)	Changi
M	,, W. R. Logan	Believed killed, Alexandra Hospital
	,, Lopez	Changi
M	,, G. H. Lowe	,,
M	,, I. MacIntosh	Thailand
M	,, J. F. Mitchell	Singapore
M	,, W. T. Quaiff	Changi
M	Surg. Lieut. J. G. Reed	Palembang
	Capt. Rodger (K.L.)	Changi
	,, C. C. Ross	Thailand
M	,, N. K. Sen	Nee Soon (?)
	,, Simpson (I.M.R.)	Thailand
M	,, R. B. Wallace	Changi
M	,, T. Wilson	,,
	,, Young	Thailand
M	Lieut. D. R. MacPherson	,,
	,, McMerine	Changi
	,, Munmery	Mukden
	,, Nicholas	Singapore
M	,, S. S. Pavillard	Thailand
	,, Poh Chee Juay	,,
M	,, P. Temlinson	,,
	,, Turner	Changi
M	,, E. C. Vardy	Thailand
M	,, L. A. Watson	,,

M = Member of Malaya Branch B.M.A.
CM = Complimentary Member of the Malaya Branch B.M.A.

His Majesty's Government has decided that inventions made in Germany since 1938 shall not be allowed to form the basis of valid applications for the grant of patents, or for registration of designs in the United Kingdom, and accordingly such applications will not be accepted by the Comptroller of the Patent Office. Any rights lawfully acquired by non-enemies before Sept. 3, 1939, in inventions for which protection was applied for in Germany in the twelve months preceding that date will be safeguarded.

Reports of Societies

HYPERTENSION

In his presidential address to the Section of Medicine of the Royal Society of Medicine on Oct. 23 Dr. T. IZOD BENNETT said that he had chosen as his subject a morbid condition which caused approximately 100,000 deaths in this country every year. His choice of hypertension was made because he felt that we were on the eve of discoveries which would give the profession control over this scourge. Progress towards control of a disease was made usually in three stages: an initial stage in which the condition became defined; a stage during which there was a large accumulation of data; and a stage in which the explanation of its occurrence was worked out—in other words, the stages of "What?" "How?" and "Why?"

It was a long time before any definition or understanding of the problem of hypertension was reached. Not until the last years of the nineteenth century was an answer obtained to the question "What?" Then followed the accumulation of a mass of material which in time answered the question "How?" The final question, "Why?" was still unelucidated, but the darkness was not entirely unrelieved. Here he referred to the brilliant experimental work of Goldblatt and his colleagues in the United States, who, a few years ago, published observations showing that when the renal arteries were partially occluded by the placing of clamps severe hypertension was caused in animals. Byrom and Wilson in this country showed in a number of experiments, extraordinary in conception and equally clever in technical achievement, that if one kidney alone was partly occluded by clamps, not only was hypertension produced but the untouched kidney underwent a change analogous to what was seen in hypertension, and if both kidneys were affected and the original clamp on one of the arteries was removed, the other kidney in certain conditions cleared up.

This work had had as one of its results the use by surgeons of unilateral nephrectomy. In a discussion on this subject in the Section of Urology in 1943 the position was usefully summarized by Dickson Wright, who concluded that "the importance of the surgical kidney in hypertension should be written down rather than up." Simultaneously with the work of Goldblatt and his followers was an important series of observations by the more purely chemical school of workers, the name most prominently associated being Page in the U.S.A. These two schools were now united; all the exponents of the Goldblatt school—or most of them—were of opinion that the effects they brought about must ultimately be due to chemical agents from the affected kidney.

As a practical physician Dr. Izod Bennett reminded the meeting that there was yet another substance which must not be overlooked—namely, potassium thiocyanate. Nobody seemed to know how this acted, but there was no doubt at all that in early cases—cases in which actual sclerosis had not yet taken place—brilliant results were occasionally achieved by its oral administration. The substance was uncertain in its action and toxic if given in too high dosage, but with careful control it could be safely used and undoubtedly afforded great relief in many cases. But he did not anticipate that thiocyanate would continue for long; soon a more logical and probably more effective treatment would be forthcoming.

The Ischaemic Kidney

Major CLIFFORD WILSON said that the ischaemic kidney might show no structural damage. Unilateral renal ischaemia would cause persistent hypertension—that was to say, failure of renal excretion and increasing load on the kidney increased by hypertension. Unilateral renal ischaemia set up acute vascular and parenchymal changes in the opposite kidney and other organs, closely resembling the lesions of malignant hypertension and chronic interstitial nephritis. In man prolonged hypertension might persist after removal of the affected kidney. The speaker showed diagrams of the technique of experiments on renal artery constriction in the rat, and photomicrographs of the damage to the kidney tissue. The most important result of this animal experimentation was that hypertension was shown

to produce lesions in the kidney which previously had been regarded as inflammatory or nephritic in origin. It also established beyond doubt the fact that malignant hypertension was an entity—there was a variety of essential hypertension which started without any apparent renal damage and later went on to renal destruction.

These observations suggested the possibility of a vicious circle in Bright's disease in which hypertension was maintained at an increasing tempo, with the result that clinically these patients suddenly got worse and went downhill. It had long been recognized that in any patient with chronic kidney disease and with hypertension the terminal histological picture was very similar. This common picture had been called chronic interstitial nephritis, and that was why chronic interstitial nephritis had been so confusing—it did not correspond to any common entity.

On the question of unilateral renal hypertension in man, there was naturally a good deal of excitement when many cases were reported in which unilateral disease of the kidneys was shown to produce hypertension and after removal of the kidney the hypertension disappeared. But after a very long search for unilateral kidney disease most people had come to the conclusion that the condition was very rare. It was very rare in cases of essential high blood pressure to be able to attribute the condition to a unilateral renal lesion, though such cases did occur and it was important to recognize them. But operation should not be performed until the intravenous pyelogram showed no excretion from one kidney and normal excretion from the other.

The Role of Renin

Dr. MARY LOCKETT discussed the role of renin and the renin-hypertensin system as an effector mechanism in experimental hypertension resulting from constriction of the renal artery. Renin had been demonstrated in the blood in many cases of experimental renal and essential hypertension, but its responsibility for the hypertension of renal ischaemia could not be said to have been clearly established by the methods of assay, and further work was needed to establish whether it was in fact the causative or the only causative agent. The results of slow renal infusion experiments raised some doubt as to whether a sustained increase of renin secretion could cause prolonged hypertension. After very considerable lowering of the blood pressure from trauma or from haemorrhage renin was shown to be liberated by the kidney and was demonstrated in the systemic blood. Although anoxaemia did not prevent the secretion of renin when the blood pressure was lowered, anoxaemia alone did not cause renin secretion. The properties of renin made it appear possible that renin was normally concerned with the regulation rather than the maintenance of normal blood pressure, and the development of tachyphylaxis gave cause for doubt whether excess of renin could be responsible for a persistently elevated blood pressure. The pressor action of renin, through the formation of hypertensin, had been shown to be due to its direct action on the blood vessels. Dr. Lockett also analysed the results of the work of investigators in North and South America and elsewhere and the varying terminology used.

Dr. W. N. ROGERS asked whether anybody had tried thiouracil in cases of hypertension. Dr. BRUCE PEARSON said that in his experience the only benefit to be seen in medical treatment of hypertension was in patients who had crises during which the blood pressure was above its usual level. Such cases showed a response to dilator drugs. Nervous people with persistently high blood pressure would respond to sedative drugs or even to the removal of a cause of chronic anxiety, but people whose blood pressure rose when they got slightly excited tended to develop permanent hypertension. Dr. THOMPSON HANCOCK spoke of the use of potassium thiocyanate. One of the important things noticed was that in the bad cases in which it was used there was very often a greatly decreased excretion of thiocyanate, and unless one was very careful in such cases one might get a very high blood thiocyanate. Dr. H. WARREN CROWE spoke of a recent paper in the *Lancet* on experimental prevention of nephrosclerosis with ammonium chloride. It was shown that desoxycorticosterone caused nephrosclerosis, and death of the rats was prevented by ammonium chloride in their drinking-water. This might account for the very satisfactory result of small doses of hydrochloric acid, given between meals, in reducing high blood pressure. Ammonium chloride broke down into urea and HCl.

Sir HENRY TIDY referred to the long period over which an organ might nurse an injury after the original cause has been removed. In renal lesions the opposite kidney might for a time show no effect and the blood pressure might fall, but once the damage had been done the effect persisted and might gradually multiply. He doubted whether the cases in which success was claimed for the unilateral operation had been observed long enough yet to say what their end might be. An example was to be found in the very large number of cases of trench nephritis in the first European war. With proper care and long rest the majority appeared to recover completely, but when a test was made on them ten years later it was found that the number showing renal deficiency had grown considerably over that shown in a previous test made about two years after the original attack. The organ once injured tended to nurse that injury and slowly to multiply its effects, and once this process had started it was likely to go on although the original cause had ceased to operate.

THE TRAINING OF CLINICAL TEACHERS

In the Section of Disease in Children (Royal Society of Medicine) on Oct. 26, Prof. NORMAN CAPON took for the subject of his presidential address "The Training of Clinical Teachers." He prefaced his main discussion with the claim that paediatricians had adhered more closely to the tenets and discipline of the science and art of medicine than any of the specialists. The conditions of their work, both personal and professional, placed them in an unusually favourable position for promoting improvements in basic medicine.

How was it possible to ensure (Prof. Capon continued) that while the student learned what was essential in factual knowledge he was at the same time given a real education, a basis on which he could continue to build from his increasing experience? It was not enough to recommend a modification of the subjects which comprised his curriculum. Moreover, there was no certainty that a lecturer busily engaged in the treatment of the sick would remind the student that some people, at any rate, were healthy. It was from the teacher that the life-blood of education flowed. He quoted the article on "Education" in the *Encyclopaedia Britannica*:

> "No doubt all education is effected through the experiences of the educated, but it does not follow that all experiences are educative. . . . It follows that an education may be good or bad, and that its goodness or badness will be relative to the virtue, wisdom, and intelligence of the educator."

Prof. Capon suggested that in a given university a small group of senior clinical teachers, aided by one of the preclinical teachers and the professor of education, should undertake preliminary discussions with a view to organizing a vacation course for junior clinical teachers. The students would be restricted to, perhaps, 15, and would come from different centres, because discussion would form an important feature of the course, and so the views and habits of different medical schools should be represented. The following aspects of medical education would need to be covered, partly as lectures and partly as discussions and demonstrations: (1) The meaning of education, with some reference to educational methods in general. (2) The scope and aims of medical education, with special reference to (*a*) undergraduates and (*b*) graduates, including general practitioners, specialists and consultants, research workers, public health workers, and demonstrators. (3) The historical approach in medical education and the value of non-medical literature (as illustrating the value of one aspect of non-medical literature he mentioned two books—Francis Thompson's *Essay on Shelley*, and *Early One Morning*, by Walter de la Mare). (4) The artistic (or humanistic) approach. (5) The scientific method, with special reference to experimental inquiry, and its clinical application, including the technique of medical examination. (6) Elementary logic, including critical analysis of evidence and assessment of values. (7) The technique of clinical teaching. (8) The place of psychology in teaching. (9) The use of medical libraries. (10) The preparation of scientific reports and papers. He next showed a list of books which might usefully be studied before

the class began—just a selection which had been of personal help to himself. They included Herbert Spencer's *Education*, Sir Thomas Browne's *Religio Medici*, Wilfred Trotter's *Collected Papers*, Sir William Osler's writings, Sir Thomas Lewis's *Clinical Science*, Francis Bacon's *Novum Organum*, W. S. Jevon's several books on logic, and many others, ancient and modern, covering the fields set out above.

It might be argued that clinical teachers had little time and sometimes little inclination for work which was not strictly technical or vocational. Lack of time was a defect likely to be corrected by a change of policy regarding teaching duties. As for lack of inclination, his own experience was that clinical teachers were most anxious to learn. Some might say that at present there were no clinical teachers who were equipped for the difficult task of conducting a course on the general lines he had just suggested. If the proposed course was to be effective it must be conducted mainly by practising clinicians, and it would be a mistaken policy to leave this basic education solely in the hands of clinicians whose experience was largely, if not solely, confined to hospitals.

The view had been put forward that teachers were born and could not be made, and that if a clinician was merely proficient in his professional work and knew his staff, the student learned by precept and example. He (Prof. Capon) gave place to no one in his respect for the first-class practising clinician ; to him he represented the highest degree of excellence attained in the profession. But they did him less than justice if they assumed that by an active effort to broaden the basis of his knowledge and to cultivate wisdom he would thereby reduce his authority. A final question was whether a teacher could learn anything worth knowing of medical education in a three-weeks vacation course. He did not claim that such a course could do more than give a viewpoint, but here was the first stone of a foundation on which the teacher could build.

Discussion

In some discussion on the address Sir ADOLPHE ABRAHAMS, speaking as one who had been a teacher for 25 years and an examiner for 15, said that a clear distinction ought to be made between the two factors which were concerned in teaching—the teaching of students to pass examinations and the teaching of students to be practitioners of medicine. There were certain geniuses in the teaching world who could sustain this dual role, but he did not believe that a teacher could be "made" ; the true teacher was naturally a teacher. No system of teaching would in itself produce the teacher-personality. He added that it was often urged that a general practitioner should be introduced from time to time to give students lessons in those particular parts of the art of medicine which were employed in general practice. He doubted whether that would be an advantage. The student who could follow the general practitioner on his rounds for a year would be in a different position, but that a general practitioner would be able to convey by ordinary teaching methods that particular skill which he possessed in the exercise of his practice seemed to him very unlikely.

Sir ERNEST ROCK CARLING suggested that they should ask themselves what kind of education they would like if they wished to be teachers of medicine. It must begin with the humanities. If a man had a real working knowledge of any of the Latin languages and literatures it was a very important beginning to his education. It was important that the teacher should understand the function of words and know what he was doing when he wrote or talked. Teachers in the future would have to do what they had not done in the past—namely, give instruction in the social aspects of medicine and in the maintenance and purpose of health. They would require a wide biological outlook on man, woman, and child. At some period in the apprenticeship which the future clinical teacher went through he must go into a place where research was conducted. He set very little value on the myriads of reports of research which were put out, particularly in America, but for some period the future teacher should work in a research laboratory. Then, since he must have the dual outlook in his teaching, he should himself have a full factual memorized knowledge of the whole literature and technical details of some small field, while no longer burdening his memory with factual material concerning the rest of medicine. Much could be learned by the teacher in travelling to clinics in this country and abroad, especially if he accompanied other teachers on such travels. This enabled him to keep right up to date in his work. Sir Ernest said that if he took on a chief assistant his plan would be for a year to lay himself out to teach him all he could ; if the pupil was satisfactory he would continue him for a second year, during which he would expect him to help him, and if at the end of that second year he was still satisfactory he would in the third year put everything on his shoulders and merely stand by. Some plan along those lines would turn out a first-class set of young people. Dr. ALAN MONCRIEFF pointed out that in future a great deal of their teaching must be given to patients or potential patients, and the more one set out upon the care of the healthy as distinct from the care of the sick the more did medical practice become a purely educational process.

Correspondence

Demobilization of Doctors

SIR,—The rate of demobilization of medical officers in the Forces will be watched with critical eyes until demobilization is complete. So far there have been two phases. The first began with the end of the German War. The Royal College of Physicians was disturbed at that time by the general assumption that the demobilization of doctors in the Services could not keep pace with the release of other members of the Forces. It was even affirmed that the war with Japan, owing to the distances from our bases and the sickness rate, would be even more of a drain on medical man-power than the conflict in Europe and North Africa had been. Representations were made to those who had the power to act, and this phase came to an end when an order was given at the highest level for the demobilization, forthwith, of 1,600 doctors (the contribution from the Army was to be 1,100).

In consequence of this order the position was transformed overnight. It was no longer necessary to argue that the demobilization of doctors ought to keep pace with the release of other members of the Forces. It only remained now to make it possible by partial replacement for the Government to carry out its own policy.

Why is the partial replacement of demobilized officers necessary ? Since the average age of Service doctors is higher than the rest of the Army, the number of doctors in the early groups for demobilization was correspondingly high. For example, when Group 11 had been demobilized 1,100 doctors were released—that is, 10% of all the doctors in the Army, while only 3% of the rest of the Army had been released ; in other words, doctors were being demobilized three times as fast as the rest of the Army. Only when Group 26 was reached would the rate of demobilization be the same for both. But it is plain that the demobilization of the R.A.M.C., particularly of specialists, cannot continue indefinitely to outpace the rest of the Army, for the simple reason that a time would come when there would be no doctors left with the Forces. To ensure, then, that doctors can be released with their age groups, recruitment by the Central Medical War Committee must go on, otherwise medical demobilization will inevitably be slowed down. Replacement will of course be less difficult if an appreciable proportion of those who are eligible for release elect to remain in the Service. Of 1,100 doctors in the Army first due for demobilization, 250 have already asked for deferment of their release.

This, however, is a detail, if an important detail, in the machinery of demobilization. The second phase began with the order to which I have referred, which ensured that the demobilization of doctors would keep pace with that of other members of the Forces. In this phase it is the rate of demobilization of the combatant ranks which determines how quickly doctors are set free. That is alleged to depend on our commitments ; it really depends on who is responsible for estimating those commitments. In time of war we are all content to leave this to the leaders of the Forces, but now that peace has come we have commitments on the home front which are even more urgent than those in occupied countries, and it

is not within the province of the heads of the Services to assess those civil needs. If doctors are to be demobilized in time to meet civil emergencies, the estimates of the number of those who must be kept under arms will have to be revised by the Cabinet itself. We need more doctors in civil practice and we need them quickly. As things stand, by the end of this year the Army and the Royal Air Force will have released a quarter of their medical officers.

For some time the Royal College of Physicians has been considering what steps can be taken to ensure that in filling vacancies on the staffs of hospitals the claims of doctors now in the Services shall not be overlooked. Most of the hospitals in this country, large or small, have not filled the vacancies which have occurred in the last six years, and at the present time a large number of posts are vacant. The fort is being held by very senior members of the profession, many of whom for reasons of health or of years feel that they cannot go on now that the war is over. There is a danger that such vacancies may be filled before doctors in the Services are demobilized, and without their applications receiving the consideration they so plainly deserve. There is no simple remedy; there are many difficulties which confront anyone who is anxious to help. For example, hospitals may wish to interview the candidates, and we must at once concede that it will not be possible to bring applicants back from the armies of occupation for that purpose, for plainly if so much is conceded to one calling it cannot be denied to others. Once, however, an appointment is made, the procedure is simple. When a Service doctor is appointed to a vacancy on the staff of a hospital that hospital applies for his transfer to Category B for immediate release. Nor can we doubt that the Central Medical War Committee would support the release of the successful applicant, since the efficient staffing of hospitals is half the secret of an efficient health service.

When should these posts be advertised? The Demobilization Committee of this College believes that the vacancies ought to be staggered; for instance, one-third should be advertised immediately, one-third in January, 1946, and one-third on July 1, 1946. If such a suggestion is thought to be too rigid a hospital might be advised to advertise a reasonable proportion of such posts now, but in fairness to practitioners serving abroad no such appointments should be made for at least four months.

It matters little who gets the credit for speeding up the demobilization of doctors. Top marks ought to go to those members of the Coalition Government who were responsible for the order to which I have referred. I do not doubt that the present Cabinet will be equally sympathetic, for the health of the country is involved. It is not within the province of the Directors-General of the Medical Services to lay down the rate of demobilization, but once that has been decided I must testify to the energy and good will with which they proceeded to put their instructions into practice. They consented readily to serve on the College Demobilization Committee and welcomed junior members of their Services as colleagues on that committee. For my part, I owe a special debt of gratitude to General Hood, who made it possible for me and Dr. H. E. A. Boldero, the Registrar of the College, to go to Germany for five days with Prof. Stanley Davidson, to discuss on the spot problems associated with demobilization with more than half the medical officers in Germany, Holland, and Belgium.—I am, etc.,

London, S.W.1. MORAN.

Pernicious Anaemia and Cancer

SIR,—In 1933 (*Acta med. scand.*, 1933, **80**, 466; *Quart. J. Med.*, 1933, n.s., **2**, 281) I discussed the association of pernicious anaemia and cancer and added 6 cases of my own to the 29 definite cases collected from the literature at that time. Your annotation (*Journal*, 1944, **2**, 503) refers to the surprisingly high frequency (12.3%) of cancer of the stomach and pernicious anaemia noted by Kaplan and Rigler from an examination of post-mortem records and by Rigler, Kaplan, and Fink from an examination of a small series of patients radiologically. On looking through my own records I find a much smaller frequency; thus, in my first 1,600 consecutive patients with pernicious anaemia who have been under regular observation and treatment in my clinic during the last seventeen years I have observed only 28 patients (1.75%) with cancer; of these 12 (0.75%; 6 males and 6 females) had cancer of the stomach after 17, 11, 14, 5, 11, 8, 13, 5, 4, 1, 2, and 1 years respectively (an average of 7.66 years), while 1 developed pernicious anaemia at the age of 58 years, five years after the diagnosis and excision of a gastric cancer. Nine of the 12 were over 60 years of age when cancer was diagnosed.

In my series also 1 male with pernicious anaemia developed cancer of the caecum six years later, 2 (male aged 58; female aged 66) cancer of the colon after nine and six years respectively, while 2 females (aged 66 and 67 years) had pernicious anaemia diagnosed simultaneously with the diagnoses of cancer of the caecum and the colon respectively. Two patients with pernicious anaemia developed cancer of the pancreas after intervals of twelve and three years respectively, and 2 patients had pernicious anaemia with cancer of the throat, mouth, or tongue, 2 involved the uterus or vagina, and 1 had a breast cancer.

A radiological examination at six-monthly intervals of 1,600 patients is manifestly impossible under present conditions, and in my opinion is not necessary when these patients are under proper and regular supervision in an established clinic. It is important to be on the constant look-out for slight changes or recurrence of symptoms in adequately controlled patients with pernicious anaemia. Thus the gradual onset of vague dyspepsia, bowel disturbances, loss of weight, or anaemia in spite of adequate treatment with preparations of liver or stomach, and with the usual hydrochloric acid-pepsin mixture, should at once suggest further investigations, not only radiologically, but for the presence of occult blood in the faeces and changes in the fractional test meal.

As I have previously observed (*Quart. J. Med.*, loc. cit.) one must also be aware of the greater frequency of other diseases developing in or being associated with pernicious anaemia. Thus in the same 1,600 patients I have seen the development of diabetes mellitus in 28 patients, and thyroid disease (hyperthyroidism or myxoedema) in 36 patients. Here again slight changes in the well-being of the patient in the early stages, with the gradual onset later of the typical symptoms and signs of these three diseases, should immediately suggest further investigations and the appropriate additional treatments.

Obviously, with the considerable prolongation of life and the good prognosis in patients suffering from pernicious anaemia who are adequately and properly treated and kept under regular observations in special clinics, it must happen that these patients, in the ordinary course of things, will be liable to develop other diseases just as the normal individual may do if he lives long enough. Nevertheless, it is important constantly to keep in mind the possibility of complications such as these, so that early and proper treatment may prevent their further development and the impairment of the proper response of the pernicious anaemia to anti-anaemic therapy. It is essential that fully equipped haematological clinics should be established so that these cases and their treatments can be properly supervised at regular intervals, in the same way that diabetic clinics operate.—I am, etc.,

Royal Infirmary, Manchester. JOHN F. WILKINSON.

Effects of Choline

SIR,—The letter from J. A. Barclay, R. G. Kenney, and W. Trevor Cooke (Sept. 1, p. 298) and your subsequent leader (Oct. 27, p. 573) on the dosage and effects of choline chloride offer us an excuse to report the following experiments and observations.

The experiments were carried out in the attempt to confirm Davis's idea that heavy dosage with choline chloride depresses the blood count, and might possibly be used as the basis of a biological test for the activity of liver extracts for the treatment of pernicious anaemia. We took three rabbits and made daily counts of the red cells, white cells, haemoglobin, and reticulocytes. After a control period of one month we began treatment with single daily doses of choline chloride, 10 mg. per kilo by stomach tube. The dosage was increased at intervals of approximately ten days to 20 mg., 50 mg., and 100 mg. per kilo, but no change was observed in the behaviour of the rabbits or the condition of their blood. After forty days the dose was therefore increased to 500 mg. per kilo, but on the fifth day of this regime one of the rabbits had a convulsion and died shortly

after receiving its daily dose of choline. There was no gross lesion post mortem, and there was no change in the blood count in any of the rabbits. Treatment was now suspended, and the other two rabbits were observed for a further fortnight, during which their blood counts remained as constant as ever.

Our conclusion from this small series of experiments was that it was unlikely that a simple method of assaying liver extracts could be devised along these lines. It is also evident that heavy dosage of choline chloride by mouth may be followed by sudden death, presumably due to stimulation of the parasympathetic nervous system. The fatal amount corresponds with a single dose of 30 grammes in man. This is not much more than the 20 grammes a day recommended by Barclay and his colleagues as the curative dose of choline chloride in man. When large amounts of choline chloride are administered in man the dose should therefore be fractionated, and the patient should be under careful supervision so that pharmacological antidotes can be administered in the event of a mishap.

We have given large doses of choline by mouth to two women with cirrhosis of the liver. A woman aged 58, with atrophic cirrhosis of non-alcoholic origin, received 5 g. three times a day for five weeks with no demonstrable effect, good or bad. A woman aged 22, with a more subacute type of cirrhosis, received the same dose, 5 g. t.d.s., p.c., but treatment had to be stopped at the end of a fortnight because of persistent nausea. In a third patient we used a much smaller dosage of choline chloride. This was a woman who contracted infective hepatitis during pregnancy and had been jaundiced for six weeks. She received choline chloride 2 g. a day for nearly a fortnight, and during this period her symptoms greatly improved.—We are, etc.,

SYLVIA M. LIVINGSTONE.
L. J. WITTS.

Oxford.

Bronchial Embolism in Tuberculous Reinfection

SIR,—I think that your annotation on the pathogenesis of tuberculosis (Oct. 20, p. 537) dismisses the importance of bronchial embolism too lightly. You refer to some observations of mine and state: "The suggestion has been made that the frequent occurrence of reinfection lesions in the posterior parts of the upper lobes may be explained by the fact that foreign material, such as iodized oil, tends to be inhaled into these areas when the patient lies on his back and that tuberculous reinfection is therefore the result of bronchial embolism." This is not quite what I said. My first point was that the lung abscess affects the posterior subapical part of the upper lobe and the apical part of the lower lobes so frequently that an explanation is needed, and that bronchial embolism seems a reasonable and correct explanation. In order to show that bronchial embolism *could* cause a lesion in these sites my colleagues, F. Hodgkiss and H. O. Jones, and I (*Guy's Hosp. Rep.*, 1942, **91**, 131) demonstrated that iodized oil could be made to enter these parts of the lung by simple posturing of the patient on his side or on his back. The association of inhalation of infected blood-clot, nasal secretions, etc., with a subsequent lung abscess is undoubted, and a parallel was then drawn with reinfection lesions in pulmonary tuberculosis. The radiological mimicry between a lung abscess and an acute tuberculous lesion may be so close that differential diagnosis may be impossible unless or until tubercle bacilli are found. This behaviour of tuberculosis is an observed clinical fact and demands an explanation. It was not my intention to suggest that it could be explained by inhalation experiments with iodized oil. These observations were made merely to show that such a mechanism is possible; the basis of the hypothesis rests on the study of numerous cases of undoubted bronchial embolism.

You also state that "it is difficult to see why the individual should be lying down when he acquires his infection." Here again I think the approach is incorrect. My argument was that it is *while the patient is lying down* that infection can occur and does occur in these parts of the lungs. That is a fact which must be accepted for further consideration and argument and is not to be dismissed by saying it is difficult to see why the individual should be lying down. You also state "a minute inhaled droplet might behave differently from a blob of iodized oil introduced artificially." I agree. The natural inference is that the mechanism of bronchial embolism is not by a "minute inhaled droplet"; the inhaled material must be larger in amount.

During working hours the larynx is on guard against inhalation of material from the pharynx; it is during sleep (and anaesthesia) that the guard is relaxed and material can enter the lungs. If the individual is sleeping in an infected room or ward, especially with a tuberculous subject, his nasal, oral, and pharyngeal mucus can certainly become infected and could then carry tubercle bacilli to the lungs. An individual exposed to infection during waking hours will sustain droplet infection of his nasal and pharyngeal mucosa; much of this will be washed away, but it is not unlikely that bacilli are held in crypts in lymphoid tissues and in other sites, and are liberated into mucus during rest and sleep and can then pass into the sites of election in the lungs. Saliva often collects in the mouth and dribbles on to the pillow during sleep; it must be equally easy for it to run back into the pharynx. Germs can reach the patient by droplet infection, but there is little evidence that they reach the lungs solely by droplet infection. I suggest the evidence points to their reaching the lungs by inhalation of gross material from the nose and pharynx, and particularly during sleep.—I am, etc.,

Guy's Hospital, S.E.1. R. C. BROCK.

Sequelae of Emphysematous Lung

SIR,—I was very interested in the article on massive surgical emphysema, pneumothorax, and pneumoperitoneum by Dr. Ronald Jones (Oct. 20, p. 530), in which he described in Case I an unusual sequence of events after intubation of the trachea with the bronchoscope. He states that after intubation there developed a surgical emphysema of the face and neck with deep cyanosis, which was associated with swollen tracheobronchial mucosa and a complete blockage of the right and a partial blockage of the left main bronchus. Four days later a radiograph of the chest and abdomen showed a right pneumothorax and pneumoperitoneum. Ten days later the pneumothorax and pneumoperitoneum had disappeared. Truly a problem at first sight.

I would venture to give a probable explanation in this case. It would appear to me that the outstanding feature was not so much possible damage to the tracheo-bronchial tree, causing a pneumomediastinum, as the blocking of the main bronchi. When this latter occurs over a short period of time there is a ball-valve action, with marked increase in the intrapulmonic pressure—i.e., more air enters the lung than can escape. Having achieved this temporary state of a ballooned emphysematous lung there are two alternative catastrophes, and both may happen: (*a*) a peripheral alveolus ruptures into the pleural space, causing a pneumothorax; (*b*) the rupture may take place through both visceral and parietal pleura in the region of the mediastinum, causing pneumomediastinum. I have seen pneumomediastinum occur in post-operative atelectasis, due to the forced expiratory coughing effect, which attempts to clear the bronchus and leads to rupture of the mediastinal pleura.

Having established pneumomediastinum the free gas or air has an interesting roving commission. It may travel in a cephalic direction or caudally: (1) in a cephalic direction as surgical emphysema into the neck, shoulders, and face; (2) caudally as two linear paravertebral gaseous shadows into the retroperitoneal space of the abdomen via the route of the great vessels through the diaphragm.

The retroperitoneal gas appears first in the subdiaphragmatic region, and radiographically and clinically is indistinguishable from pneumoperitoneum. There would be a tympanitic note in the upper abdomen, and the insertion of a needle into the flanks would allow the escape of gas. The retroperitoneal gas also passes into the flanks and perirenal region. The perirenal distribution is an important differential diagnostic factor from pneumoperitoneum. As Dr. Jones states, there is no communication between the retroperitoneal space and the peritoneal cavity, unless some obscure accident causes a breach in the peritoneum. This is unlikely. I feel that Case I was probably a marked degree of retroperitoneal gas simulating pneumoperitoneum. An interesting diagnostic test would have decided this point. In pneumoperitoneum the gaseous sites move to the

most dependent portions when the patient is changed from the vertical to the lateral decubitus, but retroperitoneal gas remains fixed in its loose areolar space.

In retroperitoneal perforations of the gastro-intestinal tract, fortunately rare, the reverse of the above retroperitoneal pathway holds good—i.e., perirenal, the flanks, subdiaphragmatic, and on with the great vessels through the diaphragm into the mediastinum and neck. An acute abdomen plus surgical emphysema spells only one thing—a retroperitoneal perforation somewhere in the gastro-intestinal tract. A more detailed description of the behaviour of retro- and intra-peritoneal gas will be found in my address to the annual meeting of the Faculty of Radiologists under the title of "Investigation of Free Gas in the Peritoneal Cavity." This has gone to press and should appear in the November issue of the *British Journal of Radiology*.—I am, etc.,

Hull. J. E. BANNEN.

Epidemic Diarrhoea and Vomiting

SIR,—The account by Drs. George Brown, G. J. Crawford, and Lois Stent (Oct. 20, p. 524) of the outbreak last spring and summer in Salford tallies closely with the epidemic experienced in the East Riding over the same period. A very rough estimate suggests that perhaps 20,000 cases occurred in this county of only some 190,000 population, with possibly 1 in 4 being seen by a doctor. Recovery in a few days was apparently invariable, so that few faecal or other specimens were sent for examination except on the request of the laboratory. Though *B. sonnei* had shortly before been readily recovered from two-thirds of cases of a local dysentery outbreak, pathogenic organisms were noticeably absent from the 28 sample specimens from this widespread condition, and no agglutinations to *B. sonnei* were found in the 12 cases examined in a mental hospital group. A questionary to a few doctors scattered over the county elicited a clinical picture like that of the above authors, with liquid diarrhoea lasting three to four days in 100% of cases, vomiting for half a day to three days in 20% to 50%, and little else but a variable abdominal discomfort. It frequently spread through a family or community group, with intervals between cases of one to five days, probably more often spreading among those in contact than among those eating from a common source.

Such considerable economic loss as there has been this year, with the possibility of recurrence as in the Philadelphia outbreak described by Reimann, which has taken place the last two autumns, makes especially opportune the study of the Salford doctors.—I am, etc.,

Driffield Emergency Hospital, York. H. F. BARNARD.

REFERENCE

Reimann, H. A., Hodges, J. H., and Price, A. H. (1945). *J. Amer. med. Ass.*, **127**, 1.

SIR,—It was with a sigh of relief that I read the article by Drs. George Brown, G. J. Crawford, and Lois Stent (Oct 20, p. 524), and my gratitude to them is great. There was the exact description of an "epidemic" which has dogged my footsteps with its endemicity for all my ten years here. In Bedworth, a town of some 20,000 people, it has flared up about once a year—not at any set season—and often trespasses a bit into a neighbouring city. At each flare-up I should guess that 10% of the total population is affected, and I think that partial immunity develops. As for diagnosis, its maddeningly exasperating bacteriological negativeness sank into me eight or nine years ago (and my early efforts, which included sending a few cases into a city hospital, apparently did not start epidemics there).

Its absolute "non-fatalness" is mirrored in the fact that my patients and I have developed for it a pet name; we call it "Bed'orth disease"—i.e., we have developed a mental immunity, greater, fortunately, than our physical immunity. I believe it has been termed locally "gastric influenza," this name being started by a medical new-comer to the district, who was, perhaps, slightly less immune. As to treatment, it was early obvious (about 1936) that the traditional preliminary dose of ol. ric. was wrong; the treatment advocated at Salford has long been used here, plus the first 24 hours with water only (no milk) and in bed.

An interesting confirmation of the theory that some immunity develops has occurred this year. A community new to the district (240 people in a resident training college) arrived on June 1. On July 2 77 of them—i.e., 33%—reported sick, all within a few hours. The outbreak ran its usual mild course, and "mental immunity" was quickly caught by infection from their M.O. (a local practitioner). The college christened their unwelcome and uninvited local guest "the plague." The probability of personal partial immunity being possible is based on myself as a patient. My first attack (1936) was the worst, and subsequent attacks have become progressively milder. My first was very acute (with vomiting) for 24 hours; as I could not lie up I was mildly afflicted thereafter for two months.

I shall be interested to hear whether other districts of England, encouraged on to paper by the Salford report, have similar past histories.—I am, etc.,

Bedworth, Warwickshire. JOYCE HENRY.

SIR,—The writers of the article on epidemic diarrhoea (Oct. 20, p. 524) are undoubtedly correct in their surmise of the universality of this harmless type of diarrhoea. It would be interesting and highly instructive to know why these epidemics of one thing and another suddenly flare up all over the place *at the same time*. There is something of a superstition among we "old chaps," that grows with further experience, that many troubles which cannot by any stretch of the imagination be regarded as infections come in waves—e.g., accidents, births, deaths, particular types of non-infectious illness. I have recently, for example, seen three cases of coma within two days. It is months since I saw one before. Is this sort of thing really coincidence? It would be extremely interesting to keep track of such happenings over a wide field for some time, to see if we get waves or epidemics of such non-infectious happenings over wide areas at the same time. I suspect this to be the case, and suggest that the matter be investigated. Should this turn out to be so, it would suggest some common factor operating over wide areas at any given time—a factor that from the magnitude of the area involved could only be something that affected perhaps the human nervous system and through it the whole body economy.

I suggest that the cause is likely to be found in Lakhovsky's theory that cosmic radiation around the 8-metre band is responsible for the supply of electromagnetic energy which drives the body and its cells, and that meteorological and other conditions which vary the intensity of these radiations greatly modify the activity and even the health of the body cells. In his book *The Secret of Life* Lakhovsky appears to make out a very good case for his theory. If such life-giving electromagnetic radiation is subject to disturbances due to meteorological conditions over a wide area, why not, in the same areas, variations in the sensitiveness of the human nervous system leading at times to accident-proneness, the onset of labour, conditions favourable to the onset of certain types of disease, etc.? Here surely is a field for investigation that gives promise of being productive of most interesting and far-reaching results, not the least of which would be to settle the question of the "old chaps'" superstitions once and for all.—I am, etc.,

HUGH L. MACKINTOSH.

Pasteurization of Milk

SIR,—As one closely associated with the milk problem in medicine I feel that the letter written by Dr. Tudor Edmunds (Sept. 15, p. 367) calls for some comment. He is suggesting that pasteurization should be only a temporary measure, the main reason given being that the development of good herds and dairies will be neglected and that people will tend to call anything that is pasteurized perfect milk. I would refer the author to Prof. G. S. Wilson's very detailed report to the British Government on pasteurization of milk (1942).

I wish to state very emphatically that even if one attains clinically perfect herds and dairies it is still impossible to have 100% perfect milk. Even in a tuberculin-tested herd, or any first-class herd, tubercle bacillus, *Bacillus abortus* of Bang, not to mention all the streptococci, may abound. Now in Great Britain all city milk is of necessity "bulked"—tuberculin-tested and non-tuberculin-tested. One infected cow can, therefore, grossly infect up to 20,000 gallons of milk, and the poor

unfortunate individual is drinking it under the impression that it is absolutely bacteria-free clean milk.

U.S.A. public health literature makes abundantly clear the great reduction that occurs in infectious diseases and tuberculosis when pasteurized milk is used. Conditions in U.S. dairies are much better than will obtain here for a great many years, if ever. The cost of pasteurization is minute when put against the cost of treating infectious diseases and tuberculosis, not to mention the damage and loss of life. The result is permanent, safe, and lasting. Pasteurized sewage is at least sterile and is probably preferable to tuberculin-tested milk with typhoid bacilli. Something practical for a change like permanent pasteurization is much better than theorizing on a beautiful scheme that gets no further than the M.O.H.'s desk.—I am, etc.,

RONALD R. J. R. CLARKE,
Assistant Tuberculosis Officer.

Tuberculosis Hospital, Machynlleth.

The Child with Frequent Colds

SIR,—I am sure most general practitioners will have read the article by Drs. D. H. Irwin and E. Frankel on the child with frequent colds (Oct. 27, p. 566) with great interest and equally great disappointment that their conclusion as to treatment is that it is "entirely a problem for the ear-nose-and-throat surgeon." Although the authors say note was taken of the environment of the children, no further reference is made to this essentially important factor save a statement that the majority of the children came from middle- and artisan-class families. What would be thought of a psychiatrist at a child-guidance clinic if in investigating cases no study was made of the children's parents and sibs and general family and social background? Yet surely the child with frequent colds is as much a victim of his family and environment as his fellow with difficult or abnormal social behaviour. Little headway will be made with the problem until it is investigated on these broader lines and a serious endeavour is made to find the source of recurrent infections and not just to study the results.

In my experience as a general practitioner children with recurrent colds and throat infections can nearly all be classified in one of two groups: (1) those who commence their infection in infancy, often below the age of 9 months, and have attacks with such frequency that they are rarely well though usually ambulant; (2) those children whose infection starts when they first attend school.

The origin of infection of children in Group 1 is usually some older member of the family with a chronically infected throat or nose. They are largely of the poorer classes, where such factors as small rooms and overcrowding accentuate the intensity of infection, but they can also be found in higher social strata. One serious result of these chronically catarrhal and coughing children is that the mothers are no longer able to judge their state of health, and will send the children to school with severe colds and in the early stages of more severe infections, such as measles and pneumonia. Many of the children in this group would be much more benefited by the removal of their parents' tonsils than their own. The children in Group 2 get their frequent colds from the large mass of infection brought to school by the children in Group 1. This is helped by old, overcrowded, and badly ventilated school buildings. As the children grow through adolescence their colds become less frequent, but many develop permanently infected sinuses. They are the parents of the next generation, and so the cycle is complete.

This vast problem has many more aspects than can be dealt with in a letter, but I think it is only by the study, not of the individual child, but of the family in its biological setting (to use the words of Dr. Innes Pearse), that progress can be made.—I am, etc.,

F. M. MALLINSON.

Sittingbourne.

SIR,—The article by Drs. D. H. Irwin and E. Frankel (Oct. 27, p. 566) appears to be a good example of modern medicine gone wrong. We are told the results of many investigations made, except the most important. Does the child normally keep its nose clear and breathe through it with the mouth shut? Apart from the statement that "in 33 cases the mothers complained that their children snored at night and were mouth-breathers," I can find no mention of this most important factor. Nor is it clear to me from the above sentence whether other children were mouth-breathers, though their mothers failed to complain about it; nor whether the mouth-breathing referred to is at night only, or in the day too; nor whether it was their usual practice or just when they had a cold. Nor is it emphasized that it is more than usually important to keep the nose clear and breathe through it when the child has a cold. The beneficial or other results of removal of tonsils and adenoids depend, too, mainly on this factor.

Lastly the authors say this "is entirely a problem for the ear-nose-and-throat surgeon, who should keep these children under supervision in a special clinic"; they might add "and teach them adequately to blow their noses." This is just the sort of thing the neglectful parents wish: they would do better to read page 165 in the current number of the *Health Education Journal*.—I am, etc.,

G. C. MILNER.

Pettswood, Kent.

Psychosomatic Reactions

SIR,—Your annotation "A New Reaction to Emotion" (Oct. 13, p. 504) asked two extremely pertinent questions. It is becoming increasingly evident on clinical grounds that the suggestions inferred from such questions are indeed "probably true," in that there are other important physiological responses to emotional stimuli; and that there are other, as yet unidentified, hormonal activities associated with such stimuli, not only on the positive side, as shown so effectively by Verney and his co-workers, but also as a (? exhaustion) hypofunctioning along the same psychosomatic train of events.

Similar associations in clinical research tend to show that the prevalent curse of non-articular rheumatism may be intimately related to a psychosomatic reaction. While it has been recognized for many years that emotional or mental stress factors are operative in rheumatic cases, it has become evident during these last two years that there is certainly a type of generalized "fibrositis" showing a typical syndrome, of which the fibrositis is but a single symptomatic unit, and a unit of a syndrome closely associated with pituitary hypofunction. That this syndrome, in history, signs, and symptoms, is so strictly similar in each of these cases points to the probable association of the pituitary, which may well have a wider significance to rheumatic cases in general.

These cases have been prominent in their numbers recently, due, it is thought, to six years of emotional stimuli in various major and minor forms from bombs to queues. Contrary to the reaction described by Verney, which no doubt occurred at the time in these cases in a typical manner during the period of prolonged stimuli, they then develop the rheumatic complaint characterized by a hypofunction of the posterior pituitary—i.e., diuresis, polyuria, polydipsia, and an indurated swelling of typical tissues due to fluid retention leading eventually to an extremely painful state, which, for want of a better term, one designates an acute rheumatic panniculitis. Features of this syndrome other than fibrositis are tiredness, lassitude, both physical and mental, spontaneous or excessive bruising on minor trauma, flatulence, menopausal symptoms in women, increase in weight, rather rapid and weak pulse, and a blood pressure rather lower than normal. Some of these features may be ascribed to exhaustion of the sympathetic autonomic nervous system, but some are more directly attributable to pituitary hormonal control.

In my cases it has been found that the antidiuretic control may be rapidly regained by giving the posterior pituitary hormone in the preparation pitressin, but without evident effect on the rheumatic symptoms. *X*-ray examination shows typically normal outlines to the fossa. It is reasonable to postulate, therefore, in reference to your question that there are other hormones of the pituitary or hypothalamus probably associated in this psychosomatic cycle of physiological events. "Directed team research" is avidly and hopefully working along such lines, and for the moment, as a result of much biochemical research upon the blood, interest is now centred round the adrenocortical hormone of the anterior pituitary.

While in a more direct way to answer your question, a similar emotional psychosomatic reaction has been delineated as a laboratory biochemical reaction upon certain hepatic functions, drawing an interesting graph, since before the fall of Singapore, which closely parallels the emotional state of the populus in its reaction to bombs, politics, and peace.

The purpose of this letter, Sir, is therefore to underline the importance of the questions you have postulated, to comment upon the real value of these annotation reviews, and to stimulate research workers on non-articular rheumatism to review the very many closely associated features recorded of pituitary function in the light of their own particular field; and to summarize by stating that in the final evaluation of the pathogenesis of non-articular rheumatism an answer will probably have to be found to the further question: What functional activity or lack of activity in the hypophysis or hypothalamus can be shown to be paramount in these severe cases of generalized fibrositis?—I am, etc.,

London, W.1. HARRY COKE.

Surgical Catgut

SIR,—I believe I am correct in saying that before the war a large proportion of surgeons in this country used catgut made by a well-known American firm. This has been unobtainable during the war, so that those of us who used this catgut have had to use English makes which we had previously found unsatisfactory. The makers in this country have had a wonderful opportunity during the war of getting their makes established as favourites with surgeons, and thus increasing their sales and maintaining them when the American make is again available.

I have yet to use a British make of catgut which can be relied upon to be consistent in size and strength: the contents of tubes of the same nominal strength are often exasperatingly variable. This inconsistency seems to have increased considerably since the numbering of the sizes was changed a little time ago. The makers are aware of this variability in strength and size, for they make excuses for it. It may be difficult to overcome, but it is possible, as the Americans have shown.

Theatre sisters and retailers of catgut have told me that they will welcome the return of the American catgut so that complaints (and curses) from surgeons about catgut will cease. Is it too much to hope that now, at the eleventh hour, the British catgut-makers will endeavour to standardize their catgut so that this commodity need not be largely imported? There has been a large return to linen and cotton thread, but catgut is still necessary in places. Why, too, cannot the English makers maintain a standard length of tube for catgut? Many tubes will not fit into surgeons' sterile catgut containers.—I am, etc.,

London, W.1. JOHN HOSFORD.

Haematuria following Injection for Varicose Veins

SIR,—Surely the haematuria which sometimes occurs following ethamolin injection of varicose veins and referred to by Mr. Harold Dodd (Oct. 13, p. 510) is ominous. I met with one such case: a patient whom I injected several times as an outpatient and who consistently got profuse haematuria within the first twenty-four hours of injection. I have never been able to get an explanation of the occurrences. I had never heard of a similar incident until I read Mr. Dodd's letter. Can he suggest the pathogenesis?—I am, etc.,

London, S.E.13. W. KEVIN BARRY.

Chin-Chin!

SIR,—Your correspondent "Four Chins" in his interesting letter (Oct. 20, p. 547) advises "any fat person to count up the calories consumed regularly every day for some years back"; this, for a layman, would prove to be a rather tedious and difficult task. About 15 or 20 years ago "slimming" was the vogue, and patients occasionally consulted me regarding their over-weight. My simple but effective plan was to give the applicant on a slip of paper his or her normal weight for sex, age, and height, with instructions gradually to reduce in amount at each meal the ordinary diet, until the normal level was reached and maintained. Advice was given as to checking the weight on a reliable machine at regular intervals, say, weekly or monthly, and recording it, any difference in weight of clothing being noted. Those of us who were in practice at the time mentioned may remember patients who, from total avoidance of certain kinds of food which they believed were "fattening," induced some degree of ill-health, and we know that adiposity may have other causes than that of overheating.—I am, etc.,

Burgess Hill. W. W. SHRUBSHALL.

Psychiatry in the Services

SIR,—I was extremely interested in the letter by Wing Cmdr. K. Bergin. I would like to add my quota of experiences and express my opinion on the subject.

There is no doubt that the responsibility as regards the disposal of all psychiatric patients lies largely in the hands of the regimental medical officer. He needs to sort out his cases judiciously and exercise care when initiating a probable series of events which might culminate in "the final scene" of invaliding. I have never had a case returned to me by the psychiatrist without a recommendation of down-grading, and this has been the experience of many of my colleagues with whom I have discussed the matter. Presumably the psychiatrist—who, after all, has only seen the man for the first time in an interview lasting about half an hour—justifiably assumes that the patient has been regularly observed by the company commander and the medical officer, and that the latter has concluded that down-grading is the only possible solution. The ultimate problem is, therefore, the determination of the category in which the patient is to be placed, and this is decided by the higher authority.

I, myself, have been very cynical of some of these cases. When a man with ? anxiety neurosis relates his symptoms I almost invariably give him a little "pep" talk and ask him to report again in a week if there is no improvement. During this time I request that the company commander keep him under his observation. If the man returns—often he does not, my initial chat with him having been apparently successful—and says that the symptoms are the same or worse, I decide from my own observations and those of the company commander whether he should be sent to a psychiatrist.

The assessment of psychological dysfunction in the Army I find extremely difficult, and many so-called sufferers are often regarded as having "got away with it," especially when their condition after invaliding has been so much improved. But I am certain that in many cases invaliding in itself is the only successful treatment. In peacetime jobs these men have been relatively happy, and their tolerance of change is inherently low. When the change is of a military nature they, consciously or subconsciously, try every means to enable them to revert to their previous mode of existence by getting themselves invalided or their medical category reduced. Failure to obtain their objective will probably act as the seed for more severe psychogenic disorder. I consider, therefore, that many of these men who have been therapeutically down-graded or returned to civil employment and have benefited therefrom are true cases of low mental tolerance. Employing them as A1 soldiers might be risking irreversible mental ill-health.

I regret that there are many who have with repulsive cunning secured a far more tolerable existence than the average soldier. But until diagnosis in psychiatry is made more exact (and the ideal is extremely difficult to perfect), these cases are inevitable and will always infest the community of true sufferers, who might by the over-sceptical medical officer be regarded as malingerers and treated accordingly.—I am, etc.,

WILLIAM BRAND,
Uckfield, Sussex. Capt., R.A.M.C. (R.M.O.).

SIR,—In these enlightened days it is indeed disheartening to read Wing Cmdr. Bergin's comments on the disposal of psychoneurotics in the Services. That he has "become very cynical of these cases" is surely to admit defeat. His catalogue of neurotic symptoms is comprehensive, but his understanding of their mechanism is unfortunately coloured by an attitude of mediaeval mysticism. He is apparently obsessed by the notion that the psychoneurotic is possessed by the devil and that he is endeavouring with this preternatural skill to outwit the poor medical officer. In truth, such a battle is the creation of a closed mind.

Very few of these patients are malingerers. And in any case, in view of his maladaptation, should we not also treat the malingerer as a psychiatric problem? One does not have to be a Sigmund Freud or invoke the awesome precepts of the Oedipus complex to appreciate how such psychoneurotic manifestations as chronic headache, dyspepsia, palpitations, bronchospasm, etc., may arise. They are the physical expression of abnormal intrapsychic tension in the absence of structural

changes. Indeed, they may be regarded as a valve-mechanism that acts as a safeguard against complete emotional blow-out.

In considering the nature of abnormal intrapsychic tension in Service personnel we may conveniently recognize two factors: (1) psychoneurotic predisposition; and (2) environmental stress. I think Wing Cmdr. Bergin will agree that Service life has its stresses. By far the great majority of people succeed in weathering this little storm, but those with neurotic predisposition "have the pants taken off them," laying them bare in all their emotional inadequacy. Such members are temperamentally unsuited to Service life, though, of course, they may be able to achieve equilibrium in some relatively quiet backwater in civil life.

It is high time we realized that emotional illness can be just as severely incapacitating as organic illness.—I am, etc.,

A. BALFOUR SCLARE,
Flying Officer, R.A.F.V.R.

Silverstone, Northants.

SIR,—Wing Cmdr. K. G. Bergin raises some interesting points which have not been dealt with by subsequent correspondence. All station and unit medical officers in the R.A.F. will sympathize with him, but it is unfair to blame the psychiatrists for becoming, as they have done, "the porter at the back door." It is true that many people who are unsatisfactory in the Services and who get invalided on grounds that are obviously not connected with loyalty are invalided on the recommendation of the psychiatrists, but it should be remembered that they were not taken in on the recommendation of the psychiatrists.

Where does the fault lie? Surely it lies with the civilization which we have built up. For years before the war, through the Press and other means, the danger of living and the necessity to conserve and preserve life had been stressed. One has only to read the advertisements of the Safety First Association, to say nothing of the advertisements of those who sold preparations for the prevention of constipation and "other evils of mankind." Self-preservation is a natural instinct. Courage or the anti-self-preservation factor is not an instinct but a factor to be inculcated. It is therefore really surprising that we have had as many courageous people as we have had.

In industry before the war it was being increasingly recognized that misfits in employment did not lead to better work or increased production. Similarly, in the Services misfits do not lead to better individual or collective work. War does bring a necessity sometimes to make pegs fit holes not designed for them. To what extent we are prepared to ram the pegs in is another matter. Other nations, perhaps, are prepared to do this by inhuman means. That, surely, is one of the things we have been fighting against. It is a problem of society and not of the Services. Admittedly it is unsatisfactory to the more loyal to see the less loyal getting out on psychiatric grounds, but this does not make these psychiatric grounds any less justifiable in the interests of the individual and of the team which their presence has been seriously handicapping. The remedy lies in better psychiatric control of our outlook and civilization. The fact that Wing Cmdr. Bergin gets good results by disciplinary action does not prove that disciplinary action is the method in any but a very small percentage of cases.—I am, etc.,

RICHARD W. DURAND.

London, W.C.2.

SIR,—Wing Cmdr. K. G. Bergin's letter interested me greatly, for I have long had somewhat similar views with regard to the methods of dealing with cases of the "escape mechanism" type described by him. I can also confirm by examples that as soon as an airman obtains his desire a startling transformation occurs and his symptoms appear to vanish completely, but I have noticed this happen in a good number of cases not invalided but whose category had been lowered to home service only. I have seen airmen, frequent attenders on sick parade, be temporarily down-graded and never be seen for the next six months, then to reappear with symptoms as soon as they realize they may be up-graded and be fit for over-seas again.

I cannot, therefore, agree that "the first step on the downward path is now taken" as soon as the medical category is lowered and the patient's oversea service restricted. In my view a temporary lowering of category to service at home enables many patients to carry on serving, and since the end of hostilities I have also been surprised at the number who express a desire to have their original category restored after their period of temporary down-grading has expired. This would appear to indicate that in the lower age–service release groups patients have come to the conclusion that they are now "safe" so far as oversea service is concerned and symptoms are no longer necessary. There is also the fact that they have to find civilian employment soon, when a "reduced" category may be a drawback.

The answer, so far as I can see, is to distinguish where possible between those showing mainly "affective" symptoms and those showing mainly symptoms of hysteria (the "escape mechanism" type), for it is this latter who apparently do well with a little more discipline and benefit more by action by the executive rather than by the medical officer. The former, in my opinion, are a bad risk and ought to be invalided before they really develop fully: it is they who will be a problem in "civvy street."—I am, etc.,

J. LAURIE, Squad. Ldr.

Tealing.

Medicine in the U.S.S.R.

SIR,—When I wrote to you on Oct. 2 in connexion with Dr. Norman Henderson's letter (Sept. 1, p. 302) I assumed that, as is usually done, Dr. Henderson would in reply put forward some facts in explanation of his statement: ". . . in the event of a full-time medical service failing (as it has to a large measure in Russia) . . ." In the *Journal* of Oct. 13 (p. 514) you printed my letter together with Dr. Henderson's reply to Dr. Culbert and Dr. Fieldman. Evidently you, Sir, took it that this reply would serve as an answer to my letter as well, and thus the question could be considered as closed. Since to me, as a Soviet doctor, this method of criticism in a medical press appears strange, I feel compelled to draw attention to the following facts.

(1) In his reply to Drs. Culbert and Fieldman Dr. Henderson did not prove by facts or basic investigation of the subject of discussion anything that would give foundation for his allegation. As such data do not exist Dr. Henderson seems to draw his own inferences from literary sources such as *Red Surgeon* by Borodin, and to refer his statement to Prof. A. Semashko's quotation: "The most popular doctors and professors are able to earn extra money from private medical practice in addition to their salary from the State." Even if the above is a quotation from Prof. A. Semashko, one wonders why Dr. Henderson wishes to bring this statement as evidence of the failure of full-time medical service in the U.S.S.R. Although private medical practices exist in the U.S.S.R. on a legal basis, that does not, to any extent, affect the efficiency of full-time medical service. Trying to escape the difficult position created by the question of full-time medical service, Dr. Henderson suddenly turns to the Institute of Experimental Medicine in Moscow, and attempts in one sentence to bring into a doubtful light the activities of this, one of the oldest and greatest medico-scientific institutions of the Soviet Union. It is quite obvious in all his inferences Dr. Henderson lacks a sober attitude to the matter in question.

(2) I have found nothing in the February, 1945, issue of the *American Review of Soviet Medicine* that would corroborate Dr. Henderson's criticism of the Soviet medical service. It therefore appears that Dr. Henderson is not correct in referring to that journal.

(3) We consider that the mutual interests of the medical workers of Great Britain and the Soviet Union deserve to be given a more serious and basic study of medical questions in both countries than Dr. Henderson does. Only then mutual help and fundamental criticism will bring even nearer the medical workers of Great Britain and the U.S.S.R.

Although you state that correspondence on this subject cannot be continued, I feel it is most necessary to have this letter published.—I am, etc.,

S. SARKISOV.

London, W.2.

*** Out of respect for Prof. Sarkisov we allow him a last word. Dr. Henderson has already had much space and must be content with the reply he has given to two other readers who challenged his statement about Russia.—ED., *B.M.J.*

Obituary

JAMES SHERREN, C.B.E., F.R.C.S.

Mr. James Sherren, consulting surgeon to the London Hospital, who died at his home, White Barn, Broadstone, Dorset, on Oct. 29 after a long illness, had a remarkable career.

He was born in 1872 at Weymouth, the eldest son of J. A. Sherren, and went to school at Weymouth College. With an inborn love of the sea and ships he left school early and served before the mast. After hard and testing experiences at sea he quitted that life to enter as a medical student at the London Hospital, winning distinction in anatomy and physiology and scholarships in medicine and obstetrics. He qualified at the age of 27, and took the F.R.C.S. in 1900. His ability soon made its mark and he was appointed demonstrator of anatomy and surgical tutor in the Medical College, and in 1902 was elected assistant surgeon to the hospital. This was at a time when abdominal surgery was making rapid progress, and Sherren, following the footprints of Frederick Treves, took every opportunity to gain experience in this branch of work. He also did pioneer research with Henry Head on the peripheral nervous system and published in 1908 a monograph on *Injuries of Nerves and their Treatment*. During the war of 1914–18 he served with the rank of colonel, A.M.S., as consulting surgeon attached to the War Office, surgeon to the King George Military Hospital and to the King Edward VII Hospital for Officers, and consulting surgeon to the Yarrow Military Hospital in Broadstairs. His war services were rewarded by the C.B.E. in 1919.

[*Lafayette, Ltd.*

At the Royal College of Surgeons of England he had been Erasmus Wilson lecturer in 1906, Hunterian professor in 1920, and Bradshaw lecturer in 1925 on gastro-jejunostomy. He examined in anatomy for the Primary Fellowship, was for a short period a member of the Court of Examiners, served on the Council of the College from 1917 to 1926, and as vice-president for two years; he examined also in surgery for the University of London and was a member of the Senate. At the age of 54, and at the peak of his surgical career, with a very large consulting and operative practice, Sherren suddenly gave up his hospital appointments, left London, and went back to sea as a ship's medical officer. When he finally settled on his estate at Broadstone he did not altogether abandon professional work but gave service for some years as surgeon to the Cornelia and East Dorset Hospital at Poole.

He joined the British Medical Association in 1906 and held office as vice-president of the Section of Surgery at the Annual Meeting in Nottingham in 1926, just before his retirement from work in London. Apart from the book on nerve injuries he contributed to Choyce's *System of Surgery* and published *Lectures on the Surgery of the Stomach and Duodenum* in 1921; he had also been joint editor of Sir Robert Hutchison's *Index of Treatment*, and wrote some of the articles in the earlier editions.

Mr. H. S. SOUTTAR writes:

The passing of James Sherren closes a unique career and removes a figure memorable in the story of the London Hospital. To go to sea before the mast, to rise in a few years to be first mate on one of the great shipping lines with a master's certificate, to leave the sea, start afresh as a medical student, and reach the surgical staff of a great London teaching hospital by the age of 30, argue ability and driving force of the rarest quality. To those of us who had the good fortune to know him at the London Hospital those were his most distinctive characters, and we still feel the power of their infection. He was a great surgeon and a great teacher, but it will always be as a real man that we shall remember him.

He qualified in 1899, and after that his progress was without parallel. After holding all three house appointments, H.P., H.S., and R.A., he became surgical registrar in succession to Henry Newland, whose career in Australia was to emulate his own, but he held this post for only one year, for in 1902 he was appointed to the surgical staff of the hospital. In spite of a very busy post he managed to find time for a research with Henry Head upon injuries to the peripheral nerves which still remains of fundamental value, but with the death of H. L. Barnard in 1908 he took up the field of abdominal surgery to which Barnard had made such brilliant contributions. He had found his true vocation, and it is as one of the creators of modern gastric surgery that his name will be remembered.

He developed a large surgical practice, but he never allowed it to interfere with his hospital work, and if his demands upon his assistants were sometimes reminiscent of his early days at sea, he never spared himself, and served his patients with a rare devotion. Yet he still had one other great passion in music, for he was a fine performer on the violin, and every week a group of professionals met at his house to spend an evening in classical quartettes. He never spoke of it to us, but those of us who knew him realized that in his life there were three great loves—the sea, music, and his work.

His manner of leaving the hospital was characteristic of the man. One afternoon when he had finished his operation list he took off his gloves, and going over to the Sister said, "Good-bye, Sister; that is my last operation." He shook hands with the others in the theatre, and went down alone in the lift. In the front hall was standing the secretary, Mr. Morris, the only man who knew he had resigned, holding his hat and coat. The hall porter came forward to turn out his light on the board, but he waved him away, stood for a moment looking at his name, and then himself turned off the switch. With one pressure of Morris's hand he left for ever the scene of all his triumphs. He had gone back again to his real love, the sea.

R. D. GILLESPIE, M.D., F.R.C.P.

Dr. R. D. Gillespie, who died on Oct. 30 at the age of 47, was physician for psychological medicine at Guy's Hospital and had identified himself with the York Clinic there. He became civil consultant to the Royal Air Force in September, 1938, entered the R.A.F.V.R. on the outbreak of war, and relinquished his commission as air commodore on account of ill-health last April. At the end of 1941 he went to the United States to report to the American medical profession on the psychological effects of air warfare on the British people, and gave the Salmon Lectures in New York on psychoneuroses from the standpoint of war experience. He lectured also in Canada, and after his return described in this *Journal* (for which he often wrote) a survey by the American Committee for National Morale under the heading "German Psychological Warfare" (April 4, 1942). In the B.M.A. he served as secretary of the Section of Mental Diseases at the Annual Meeting in Edinburgh in 1927 and vice-president of the Section of Neurology and Psychological Medicine in 1937; he was also a member of the Committee on Mental Health set up in 1938.

[*Press Portrait Bureau*

Prof. D. K. HENDERSON writes:

The untimely death of Robert Dick Gillespie has come to all of us who have been closely associated with him as a shattering blow. There has been removed from our midst a man who was at the very height of his powers, a man who had made many brilliant contributions to psychiatry, and who was loved and respected by all those he had inspired and influenced. At this time it is difficult to collect one's thoughts, so as to do

adequate justice to his attainments and to determine the place he had come to occupy, both in this country and abroad, among his medical colleagues. I had known and been associated with him throughout his entire professional career. The bonds between us were very close, and I pay my tribute to his memory and his accomplishments with a sore heart.

He was born and brought up in Glasgow, was educated at Hutcheson's Grammar School and at Glasgow University. Both at School and at the University he greatly distinguished himself, gained many honours, and easily outdistanced the majority of his fellows. After graduating M.B., Ch.B. in 1920, he held appointments as house-physician and house-surgeon with Prof. Stockman and Mr. Edington at the Western Infirmary, Glasgow, and there laid the foundation of a brilliant clinical career. While he was always interested in the problems of general medicine, yet he was more especially attracted to the study of psychiatry and decided to devote himself to it. I can remember as if it were yesterday the day on which he came to see me to ask my advice as to how he might organize his future course of study. He was in the garden of my house in Glasgow and eagerly inquired what books he should read so as to prepare himself for his future work. He was rather puzzled and taken aback when I told him that what I wanted him to do in the first place was to apply himself to the care and management of patients and to base his reading on his practical experience.

I was delighted to appoint him as an assistant physician on the staff of the Glasgow Royal Mental Hospital, Gartnavel. From the first he revelled in his work, his interest grew, he gained the confidence of patients and staff, and showed his ability to formulate his views and express them cogently. I know of no one who could so readily and aptly translate his thoughts to writing; literary expression never seemed to cause him the slightest trouble. It was during that period in July, 1923, that we first collaborated as co-authors and published a paper in the *American Journal of Psychiatry* dealing with the problems of the care and treatment of ex-Service men incapacitated mentally as a result of their war experience. That was a problem very near his heart because he had been bitterly disappointed when he was not considered physically fit for military service. At the end of his clinical training with me he applied for and obtained a McCunn Scholarship in the Department of Physiology in Glasgow University. While there he applied his mind to the problem of fatigue, produced an interesting and original thesis, and was awarded a gold medal for the degree of Doctor of Medicine, Glasgow University.

We had often discussed the advisability of increasing his experience by visiting the United States. To enable him to do so he was awarded a Henderson Research Scholarship and was appointed assistant psychiatrist on the staff of Prof. Adolf Meyer at the Henry Phipps Psychiatric Clinic, Johns Hopkins Hospital, Baltimore. This was an absorbing period in his life. He enjoyed his work there, he made many friends, he was ever grateful for the kindness and hospitality which he received. On his return to Glasgow and while a further appointment was pending I asked him to collaborate with me in writing a *Textbook of Psychiatry* so as to embody and introduce into British psychiatry the psychobiological viewpoint of Adolf Meyer which both of us had found so stimulating. The first edition of our textbook was published in 1927, and ever since that time we have been in the closest association. The success which that book has attained has been due in great measure to Gillespie's enthusiasm, his literary ability, and the apparent ease with which he kept so fully in touch with all the rapid advances in psychiatric work. He had a sure and accurate touch, a fine critical sense, clinical insight to an exceptional degree, and he inspired the confidence of patients and students so that as doctor and teacher he gained the respect and friendship of all associated with him.

In the meantime he was awarded the Pinsent-Darwin research scholarship in mental pathology, University of Cambridge, and was also for a time assistant physician at the Cassel Hospital, Penshurst, Kent. While there he became an ardent admirer of the late T. A. Ross and was influenced considerably by Ross. In 1929 he was called to succeed Sir Maurice Craig as physician for psychological medicine, Guy's Hospital, and lecturer in psychological medicine, Guy's Hospital Medical School. A mere statement of the appointments held, the honours which came to him, and the busy consulting professional life which he led all packed into a short life would have been sufficient to test the endurance of the physically strong among us. Many of us wondered how his slight frame could endure the strain as well as it did, because he never spared himself and responded generously to all the claims which were made upon him. During all this time, in addition to his hospital work and his life as a consultant, he was writing and publishing articles of outstanding merit. His papers, for instance, dealing with hypochondriasis, the association of anxiety states with skin diseases, the psychology and psychopathology of childhood, constitutional psychopathy, and numerous others indicated a fertile mind and easy pen which are given to few. He also found time for more extensive contributions, and his books dealing with *Sleep* (1929), *The Mind in Daily Life* (1933), and his memorable Salmon Lectures delivered in the United States in 1941 on the *Psychological Effects of War* show the breadth of his viewpoint, his interest in social medicine, and his sympathetic understanding of those whom he delighted to help.

During the war years he willingly sacrificed his wide professional and personal interests to serve in the Royal Air Force, and attained the rank of air commodore. I know that his advice was widely sought and approved of and that his judgment was at all times greatly valued. Towards the end of this war period he saw one of his dreams come true—the completion and opening of the York Clinic, Guy's Hospital, the first psychiatric clinic in this country in association with a general hospital. If he had been spared I believe that he would have made this clinic into one of the most valuable training grounds for psychiatry.

Apart from the professional side I would like to take this opportunity to testify to the man himself. Robert Gillespie devoted himself heart and soul to the work which he loved, and in consequence it was not easy to get into touch with his underlying personality. He was handicapped to a considerable extent by his general physique, which was never robust, but yet in his younger days I remember the enthusiasm he showed and the interest he took in captaining the Cricket XI at the Glasgow Royal Mental Hospital. He was a bowler with a deceptive swerve which enabled him to outwit the opposing batsmen and take many wickets. He enjoyed those Saturday afternoon matches and allowed himself to be spontaneous to a degree which many of his later-day colleagues would hardly credit. He had wide cultural interests, was well read in all branches of poetry and prose, and in conversation had a ready and nimble wit which could relieve many an awkward situation. We had many happy times together which it will be a never-failing pleasure to recall, but his passing leaves a blank which it will never be possible to fill. A great man, a loyal friend, and a psychiatrist whose achievements will always do honour to British medicine.

Dr. FREDERICK WILLIAM KERR TOUGH, who formerly practised at Leith, died in the Western General Hospital, Edinburgh, on Oct. 24. He had studied medicine in Edinburgh, took the Scottish triple qualification in 1901, and was admitted a Fellow of the Royal College of Surgeons of Edinburgh in 1903 after serving as a civil surgeon in the South African War. During the war of 1914–18 Dr. Tough held a commission as major in the R.A.M.C., and later received the Territorial Decoration. On returning to civil life he set up in practice at Leith, where he had been house-surgeon at Leith Hospital. During the second world war he held a commission as major in the Canadian Army, in which one of his two sons is serving. Dr. Tough joined the British Medical Association in 1908 and was a member of the Scottish Committee in 1931–2.

News has been received from Barbados of the death of Dr. STANLEY BRANCH, who had been a member of the B.M.A. for 45 years. He died on Aug. 6, after a very brief illness, at the age of 73. He was fourth in the direct line of a family of medical men, and one of his sons, Dr. Gordon Branch, of the West African Government Service carries on this tradition to the fifth generation. Stanley Branch received his medical training at the University of Edinburgh, graduated M.B., Ch.B. with honours in 1898, and took the D.T.M. of Liverpool some years later. After holding appointments in St. Lucia, Uganda, and St. Vincent, he was seconded as director under the International Health Board campaign for eradication of hookworm disease in St. Lucia, and held that post until 1924, when he was appointed chief medical officer and health officer for St. Vincent, retiring in 1935. His four sons were all in the Forces during this war, and two are still serving.

Universities and Colleges

UNIVERSITY OF LONDON

Dr. G. A. H. Buttle has been appointed to the University Chair of Pharmacology tenable at the College of the Pharmaceutical Society of Great Britain as from Oct. 1, 1945. For the past six years he has been officer commanding Base Transfusion Unit M.E.F. and 21st Army Group, and latterly consultant for blood transfusion to the War Office.

The title of Reader in Nutritional Science in the University has been conferred on Gladys A. Hartwell, D.Sc., in respect of the post now held by her at King's College of Household and Social Science.

London School of Medicine for Women

Miss Elizabeth Bolton, C.B.E., M.D., B.S., has been elected to the office of President of the London (Royal Free Hospital) School of Medicine for Women on her retirement from the office of Dean. Miss Katharine Lloyd-Williams, M.D., B.S., has been elected Dean in succession to Miss Bolton. The Hon. John Mulholland has been elected Honorary Treasurer of the School in succession to Lord Rennell.

UNIVERSITY OF WALES

The following candidates at the Welsh National School of Medicine have satisfied the examiners at the examination indicated:

M.B., B.Ch.—*Hygiene:* T. J. Anthony, A. H. Beynon, Norah C. Curran, D. K. L. Davies, G. E. Davies, G. R. Davies, E. D. Edwards, Gwenllian M. Griffith, D. B. Harries, J. M. Hughes, A. S. Jones, Nest Jones, R. L. H. Jones, Mary Lawrence, Bernice G. Lewis, J. M. Lewis, R. Medlicott, Goronwy Owen, Barbara M. Parker, B. F. Richards, Beryl P. Roberts, K. I. Roberts, Jean T. Smith, S. L. Stone, W. D. C. Thomas, J. G. Tomkins, D. M. Watkins.

ROYAL COLLEGE OF SURGEONS OF ENGLAND

The Earl of Athlone and Princess Alice visited the Royal College of Surgeons in Lincoln's Inn Fields on Oct. 26, and, after taking luncheon with the President and members of the Council, toured the buildings and inspected plans for rebuilding the damaged parts.

Twelve surgery lectures will be delivered at the College (Lincoln's Inn Fields, W.C.), beginning on Monday, Nov. 12, and ending on Thursday, Dec. 6, at 5 p.m. on each day. Details will be published in the diary column of the *Supplement* week by week. Fellows and Members of the College will be admitted free of charge, but a fee of £2 2s. is charged in the case of others. Tickets may be obtained from the secretary of the College.

The following candidates were successful in the recent Primary Fellowship Examination: D. Aiken, C. E. Baker, E. V. Barling, A. E. Bartolo, J. Borrie, A. Brown, J. Davies, D. H. L. Evans, C. O. Fung-Kee-Fung, R. H. Gardiner, J. L. Gild, G. W. V. Greig, G. F. Homer, J. P. Jackson, R. N. Jones, A. Klidjian, D. W. Lacey, J. W. McNamara, G. E. Mavor, F. T. Moore, T. K. Morgan, G. B. Morris, K. W. Priddis, R. G. Robinson, W. J. W. Sharrard, R. H. Shephard.

ROYAL COLLEGE OF OBSTETRICIANS AND GYNAECOLOGISTS

Prof. Leonard Parsons, M.D., F.R.C.P., F.R.C.O.G., will deliver the sixth William Blair-Bell Memorial Lecture, entitled "Ante-natal Paediatrics," on Friday, Nov. 23, at 5 o'clock in the library of the College House, 58, Queen Anne Street, London, W.

The following have satisfied the examiners and have been awarded the Diploma of the College:

D. W. Bentinck, K. J. R. Cuthbert, Ada I. Date, Ann Donnelly, W. Donovan Mary Downey, Mary M. Essex-Lapresti, Margaret Fitzherbert, S. F. Hans J. O. Harrison, D. W. James, P. S. Jaikaran, G. T. Johnson, J. C. Miller, G. W. Mills, Marjorie B. Morton, D. C. Mundy, Diana J. Myott, J. R. Owen, Ada V. V. Parkes, A. J. Partridge, S. S. F. Pooley, D. P. Jones, O. A. Schmidt, Victoria M. D. N. Shaw, L. S. Stephens, C. E. R. Wood, G. Worden, and J. H. Young.

CONJOINT BOARD IN SCOTLAND

The following candidates, having passed the final examinations, have been admitted L.R.C.P.Ed., L.R.C.S.Ed., L.R.F.P.&S.Glas.:

M. Amin, J. Batchelor, E. J. Bates, G. H. F. Beith, C. K. Black, R. Burns, W. K. Christopher, S. J. Gelman, J. N. Gould, J. A. Gowans, Hilda Granat, D. Groad, S. Hillman, Ll. B. P. Jones, H. R. Levine, S. G. Loh, D. W. A. McCreadie, I. A. MacGregor, A. B. Mackenzie, A. A. R. Meek, A. S. Mitchell, A. Moselhi, F. A. Murray, W. M. Murray, S. R. Orens, A. C. Parry, A. G. Pollacchi, Margaret MacL. Roake, J. N. Robertson, D. O. Stonewall-Payne, W. R. Taylor, Y. E. Zaki.

L. Nabel, M.D., graduate of a recognized foreign university, was also admitted a Licentiate.

R. B. Logue and J. F. H. Hanson (*Amer. Heart J.*, 1945, **30**, 205) record a case of complete heart-block occurring in the pre-eruptive stage of rubella in a man aged 23. The heart-block was of short duration, and at the end of two weeks the conduction time had returned to normal and the infection had subsided.

The Services

Col. W. E. R. Dimond, C.I.E., O.B.E., I.M.S., V.H.S., Major-Gen. D. V. O'Malley, O.B.E., I.M.S., V.H.S., and Col. R. N. Khosla, O.B.E., I.M.S., have been appointed Honorary Surgeons to the King in succession to Cols. W. C. Spackman, I.M.S.(ret.), and B. C. Ashton, C.B.E., I.M.S. (deceased), and Major-Gen. H. J. M. Cursetjee, C.S.I., D.S.O., I.M.S.(ret.), respectively.

Temp. Acting Surg. Cmdr. A. C. K. Yates, R.N.V.R., has been awarded the D.S.C., and Temp. Surg. Lieut. P. H. Cardew, R.N.V.R., has been mentioned in dispatches for gallantry, skill, and great devotion to duty while serving in one of H.M. ships during the operations performed in collaboration with the U.S. Pacific Fleet in the capture of Okinawa and the Nansei Shoto area, over a period from March 26 to April 20, 1945.

Major-Gen. Sir P. S. Tomlinson, K.B.E., C.B., D.S.O., retired pay, has been appointed Colonel-Commandant in succession to Major-Gen. J. W. West, C.B., C.M.G., C.B.E., retired pay, who has attained the age limit for the appointment.

Major (Temp.) D. L. Charters and Capts. A. L. Cochrane and R. F. K. Webster, R.A.M.C., have been appointed M.B.E. (Military Division) in recognition of gallant and distinguished services while prisoners of war.

Capts. J. H. Patel and V. K. Pillay, I.M.S., have been mentioned in dispatches in recognition of gallant and distinguished services in the field.

Capt. J. G. Lawson, R.A.M.C., has been mentioned in dispatches in recognition of gallant and distinguished services in the field.

It was announced in the *Journal* of Aug. 4 (p. 168) that Major (Temp.) W. M. Macleod, R.A.M.C., and Capt. R. Ramachandra, I.A.M.C., had been mentioned in dispatches in recognition of gallant and distinguished services in Italy. These names should have read Major (Temp.) W. Macleod, R.A.M.C., and Capt. R. R. Rao, I.A.M.C.

Freed in the Far East.—Major H. G. G. Robertson, R.A.M.C.

CASUALTIES IN THE MEDICAL SERVICES

Died.—Capt. James Raymond Dunn, R.A.M.C.

Died at a prisoner-of-war camp in Malaya.—Major John White, I.M.S.

Died at Changi Prisoner-of-war Hospital, Singapore, of beriberi.—Capt. John Diver, R.A.M.C.

Previously reported missing at Arnhem, now officially presumed killed in action.—Capt. Percy Louis, R.A.M.C.

Missing from Sumatra end of Feb., 1942.—Surg. Cmdr. T. C. Stevenson, R.N.V.R.

Medical Notes in Parliament

Christian Science "Nurses"

Mr. LINSTEAD on Oct. 17 moved that the Nurses Regulations (1945) Order made by the Minister of Health under Proviso (b) of Subsection (1) of Section 6 of the Nurses Act, 1943, be annulled. He said that it was an offence for any person not a registered nurse to use the title "nurse" either alone or in combination with any other word. There was a proviso under which the Minister could make regulations authorizing the use by specified persons of specified names or titles containing the word "nurse." Under that proviso the predecessor of the present Minister of Health made a regulation providing that any person recognized by the Church of Christ Scientist as qualified for employment by members of that body might use the title of "Christian Science Nurse." He made no reflection on the Christian Science Church and had seen the authorities of that Church. These explained that before the war trainees were sent to America for the course at a Christian Science school followed by experience in a Christian Science nursing home. Since the war their training had consisted of experience for nearly two years in a Christian Science nursing home. There was no preliminary education, qualification, or examination. There was no statutory register and no oversight of their work. In the case of State-registered nurses in this country there were a high preliminary qualification, certificates of examination, a set course of training, and a State register with disciplinary control by the General Nursing Council. Far less than justice was done to the State-registered nurse by allowing this other group of practitioners to use the name and title of "nurse." When in the Public Health Act, 1936, Section 193 gave an exemption in respect of Christian Science nursing homes

this was only done on condition that they were not called nursing homes. He suggested the application of that precedent so that if Christian Science nurses were exempted it should be on condition that they described themselves as "attendants on the sick" and not as "nurses." Mr. MESSER seconded the motion, saying that the General Nursing Council and the Royal College of Nursing both opposed the exemption of Christian Science nurses.

Mr. ALFRED EDWARDS opposed the motion and said the matter had not been slipped through the House. There had been a thorough investigation and a special reference to Christian Scientists which had caused the House to insert the name "Christian Science Nurses." He said these nurses had a thorough training, and a large proportion of them were trained medical nurses who had become Christian Science nurses. They must be recognized like other nurses by a competent body, and there could be no confusion as each must use the title "Christian Science nurse."

Mr. ANEURIN BEVAN said the merits of Christian Science were not at issue. The House was discussing whether in the interests of legal clarity and the nursing profession a people who had not the qualifications which nurses professed should be described as "nurses." The lack of nurses in this country had reached a crisis and was not due entirely to the existence of this regulation. Nevertheless nurses had represented to him that they regarded this exception as one which derogated from the status of their profession. He asked the mover of the motion to withdraw it, and suggested a meeting for the purpose of considering whether a form of words could be found which would be agreeable to all parties. He was bound to say that the concession must come from the advocates of Christian Science, and that if there was to be a modification that modification must be at the expense of those words. After such a discussion he could easily make an amendment to the Regulation. No one would lose his rights and the Regulation could always be annulled. He would take steps to see that organizations which ought to be consulted were consulted.

The motion was then withdrawn.

Paper for Book Publishers

Mr. JOHN WILMOT announced on Oct. 22 that the allocation of paper to book publishers generally was being raised as from next month to 65% of their pre-war consumption. He could not say what tonnage of this was used for educational books. The Board of Trade also administered a special reserve to make possible the publication of important books which would not otherwise be produced. A large proportion of this reserve went to educational books.

Infant Mortality in Malta

Mr. HYND stated on Oct. 24 that the infant mortality rate in Malta, which rose during the first years of the war to the maximum figure of 345 per 1,000 in 1942, declined sharply last year to 116 per 1,000—a rate substantially lower than that of the best pre-war year, 1936, when it stood at 190. The rate up to the end of September this year was 154. The lower rate of 1944 was attributed to the fact that there was a substantial increase in the birth rate that year with a large number of first babies. Mr. Hynd suggested that these tended to receive more care and attention from their mothers than their successors. In Malta tinned evaporated milk was reserved exclusively for babies and was rationed. Full-cream milk powder was rationed to children from 19 months to 5 years.

Shortage of Doctors

Mr. AYLES reported on Oct. 25 that the supply of doctors for the urban district of Hayes, Middlesex, was one for every 4,000 inhabitants, and in the near future would be reduced to one for every 5,000 inhabitants. Mr. BEVAN replied that the medical position in this area was difficult. He was in communication with the Central Medical War Committee, which was going into the matter with the local committee. He was doing all he could to increase the supply of doctors for civilian purposes. He hoped there would shortly be a very substantial increase.

Medical "Call-up" and Release

On Oct. 29 Mr. CALLAGHAN asked the Minister of Health to accelerate the rate of demobilization of doctors by calling up newly qualified medical men when they had completed six months' house appointment, by reviewing the cases of those who had been passed medically unfit, and by calling up those medically fit doctors who had remained in civilian life although in the calling-up age limits. Mr. BEVAN said newly qualified medical men were already being called up for the purpose stated. Others up to the age of 40 were being called up to the limited extent required to provide substitutes for certain medical officers whose release was contingent on replacement. The prevailing shortage of doctors in civil practice would not permit a more general recruitment as suggested in the question. Any revision of the standard of medical fitness for the Forces would be a matter for the Ministers in charge of Service Departments.

Compulsory Vaccination

Mr. PETER FREEMAN inquired on Oct. 25 whether the Minister of Health would consider abolishing the compulsory powers of the Vaccination Acts. Mr. BEVAN announced that this was among the matters under examination in connexion with the proposals for a comprehensive health service.

M.O.s in the Army

Mr. LAWSON stated on Oct. 30 that the number of doctors serving in the Army on D Day was 11,328. The number serving at the present time was 10,091. There had been a net reduction of 510 during the month of October.

Future of the Temporarily Registered

Mr. SOMERVILLE HASTINGS on Nov. 1 asked if any decision had been reached as to the cancellation of the temporary registration of alien doctors practising in this country; and whether Mr. Bevan had considered the position of the anti-Nazi Sudeten doctors and others who had no country to which to return. Mr. BEVAN said the Government had considered these matters in connexion with the Bill foreshadowed by the Home Secretary for keeping alive for a transitional period certain Defence Regulations which would otherwise lapse in February.

Navy Demobilization

Mr. W. J. EDWARDS said on Nov. 1 that although a number of doctors in the Royal Navy were anxious to take up civilian practice, Mr. A. V. Alexander could not accept the suggestion that there were doctors in the Navy who were redundant. Doctors would be released as rapidly as possible. Age-and-service group 20 should be reached by the end of this year, and up to Group 30 by the end of June, 1946.

Osteopathy and Further Education Grants

Mr. GEORGE ISAACS disclosed on Nov. 1 that after consultation with Mr. Bevan it has been decided that osteopathy was not a profession for which grants should be given under the Further Education and Training Scheme.

Medical News

Sir John Boyd Orr, M.D., F.R.S., Member of Parliament for the Combined Scottish Universities, was elected Rector of Glasgow University on Oct. 20. He stood as Independent and received 597 votes, the other two candidates, Lord Lovat and Sir Thomas Beecham, receiving 317 and 203.

A meeting of the London Association of the Medical Women's Federation will be held at B.M.A. House (Tavistock Square, W.C.) on Tuesday, Nov. 13, at 8.30 p.m., when Dr. Kathleen Harding will speak on "Physio-psychological Aspects of Pregnancy and Labour."

The Benjamin Ward Richardson Lecture will be delivered by Mr. W. Brennan de Vine, F.R.C.V.S., before the Royal Sanitary Institute (90, Buckingham Palace Road, S.W.) on Wednesday, Nov. 14, at 2.30 p.m. His subject is "The Economic and Public Health Advantages of Centralized Slaughtering."

The Food Education Society (29, Gordon Square, W.C.1) announces that Dr. E. A. Hamilton-Pearson will give a lecture on "Food and Behaviour" at the London School of Hygiene and Tropical Medicine, Keppel Street, on Thursday, Nov. 15, at 3 p.m.

A meeting of the Middlesex County Medical Society will be held at the Central Middlesex County Hospital, Park Royal, on Friday, Nov. 16, at 4.30 p.m.

As part of the 50th anniversary celebrations of the discovery of *x* rays an additional meeting is to be held to-day (Friday, Nov. 9) at 8 p.m. at the Royal College of Surgeons, Lincoln's Inn Fields, W.C., with Sir Alfred Webb-Johnson, Bt., in the chair. Prof. Sidney Russ, D.Sc., honorary secretary of the British *X*-ray and Radium Protection Committee, will speak on dangers associated with the use of *x* rays, and Prof. H. J. Muller, D.Sc., of Amherst College, Massachusetts, on genetic effects in relation to *x* rays and other ionizing radiations.

Alderman Charles Key, Parliamentary Secretary to the Ministry of Health, said on Oct. 31, in opening the Nursing Exhibition at Liverpool, that the Minister of Health is giving his close personal attention to the supply of nurses and midwives and is expecting in the near future to make a statement about the whole position.

The Nutrition Society announces a whole-day conference on the training, qualifications, and functions of dietitians to be held at the London School of Hygiene and Tropical Medicine, Keppel Street, W.C., on Saturday, Nov. 17, beginning at 11 a.m. Papers will be delivered by Prof. S. J. Cowell and Miss R. Pybus, The Training of Dietitians: the Current Position; (*a*) Miss M. C. Broatch and (*b*) Lieut.-Col. A. N. Childs, The Function of Dietitians and their Role for the Future—(*a*) General, and (*b*) with Special Reference to U.S.A. and Canada; (*a*) Dr. C. F. Brockington, (*b*) Mr. F. Le Gros Clark, (*c*) Miss F. C. R. Brown, and (*d*) Miss J. I. Mills, Scope of Work of Dietitians in Specialized Groups—(*a*) Public Health, (*b*) Schools, (*c*) Industry, and (*d*) Hospitals. Non-members wishing to attend should apply to a member of the Society for a ticket of admission. Further details may be obtained from the Honorary Secretary, Dr. L. J. Harris, Nutritional Laboratory, Milton Road, Cambridge.

The Medical Research Council has appointed the following to be members of its Industrial Health Research Board until Aug. 31, 1948: Prof. F. C. Bartlett (chairman), Dr. A. N. Drury, Prof. T. Ferguson, Lord Forrester of Corstorphine, Prof. A. Bradford Hill, Dr. Donald Hunter, Prof. Esther M. Killick, Prof. R. E. Lane, Prof. J. M. Mackintosh, Dr. E. R. A. Merewether, Prof. J. A. Ryle, and Mr. J. L. Smyth. (Dr. R. S. F. Schilling, seconded to the Council's headquarters staff from the Factory Department of the Ministry of Labour and National Service, continues to be secretary of the Board.)

EPIDEMIOLOGICAL NOTES

Discussion of Table

In *England and Wales* whooping-cough, acute pneumonia, and diphtheria increased in prevalence. The notifications rose in number by 117, 103, and 39, while those for scarlet fever fell by 81.

The diminution in scarlet fever was principally in the northern counties. The largest variations in whooping-cough were a rise in Southampton of 54 cases, and a fall in Warwickshire of 30. The increase in the incidence of diphtheria was contributed by only a few counties; the largest rises over last week's totals were 13 cases in Surrey, 12 in Warwickshire, and 10 in Gloucestershire.

The only fresh outbreak of dysentery was in Hertfordshire, Harpenden U.D. 12 cases. The other large returns were London 50, Lancashire 40, Surrey 32, Glamorganshire 17, Kent 15, Middlesex 14, Warwickshire 13.

A case of smallpox has been reported in London. The patient was a soldier returned from abroad.

In *Scotland* the only variation of note in the returns was an increase of 34 in the incidence of scarlet fever. The notifications of diphtheria in Glasgow increased by 16. The largest returns of dysentery were Glasgow 40, and Edinburgh 19.

In *Eire* the notifications of diphtheria were 20 fewer, but the incidence of measles rose by 47, and of diarrhoea and enteritis by 27. Measles was mainly confined to Dublin C.B. 51, and Dublin North U.D. 34, the remaining 7 cases appearing in six registration areas. Sixty-one cases of diarrhoea and enteritis were notified in Dublin C.B.

Diphtheria

A study of the recent returns of diphtheria suggests that a large continuous reduction in incidence, which seemed likely from the trend of the disease during the first months of the year, will probably not occur. Since 1942 the successive seasonal rises have decreased and the autumn level has been lower than that of the preceding spring rise, but this may not characterize the returns of this year. The following table gives a comparison of the returns for the most recent period and the first weeks of the year with the corresponding periods of the preceding year.

Weeks	Notifications			
	1942	1943	1944	1945
1–13	11,658	10,799	9,005	6,061
29–42	10,988	8,952	6,873	6,206

The incidence of diphtheria may be reverting to the size of that of a year or two ago, although it is too early to state whether the increase has any real significance or is just a chance occurrence. The present trend is disappointing.

Week Ending October 27

The notifications of infectious diseases in England and Wales during the week included: scarlet fever 1,754, whooping-cough 1,029, diphtheria 576, measles 422, acute pneumonia 547, cerebrospinal fever 35, dysentery 247, paratyphoid 5, poliomyelitis 27, typhoid 12.

No. 42

INFECTIOUS DISEASES AND VITAL STATISTICS

We print below a summary of Infectious Diseases and Vital Statistics in the British Isles during the week ended Oct. 20.

Figures of Principal Notifiable Diseases for the week and those for the corresponding week last year, for: (a) England and Wales (London included). (b) London (administrative county). (c) Scotland. (d) Eire. (e) Northern Ireland.

Figures of Births and Deaths, and of Deaths recorded under each infectious disease, are for: (a) The 126 great towns in England and Wales (including London). (b) London (administrative county). (c) The 16 principal towns in Scotland. (d) The 13 principal towns in Eire. (e) The 10 principal towns in Northern Ireland.

A dash — denotes no cases; a blank space denotes disease not notifiable or no return available.

Disease	1945					1944 (Corresponding Week)				
	(a)	(b)	(c)	(d)	(e)	(a)	(b)	(c)	(d)	(e)
Cerebrospinal fever	39	3	22	—	—	36	1	19	3	1
Deaths		—	—				1	—		
Diphtheria	604	40	161	76	15	580	24	175	153	25
Deaths	7	1	—	—	—	7	—	5	1	—
Dysentery	284	50	87	1	3	391	57	110	3	—
Deaths				—	—				—	—
Encephalitis lethargica, acute	4	1	1	2	—	1	—	2	—	—
Deaths		3					1			
Erysipelas			42	12	2			63	14	5
Deaths		—					—			
Infective enteritis or diarrhoea under 2 years				81					39	
Deaths	57	3	16	11	6	50	3	19	13	1
Measles*	441	40	70	92	—	3,779	54	253	33	106
Deaths	—	—	1	1	—	2	6	—	—	—
Ophthalmia neonatorum	66	8	14	—	—	73	6	16	—	1
Deaths										
Paratyphoid fever	7	1	1 (B)	—	1 (B)	6	1	—	—	—
Deaths	—	—	—	—	—	—	—	—	—	—
Pneumonia, influenzal†	518	32	7	1	2	640	40	5	1	3
Deaths (from influenza)	11	3	1	—	—	25	4	4	3	—
Pneumonia, primary			154	15				194	21	
Deaths		24		4	4		28		11	5
Polio-encephalitis, acute	2	—				—	—			
Deaths		—					—			
Poliomyelitis, acute	28	3	6	4	1	14	—	5	3	1
Deaths		—					—			
Puerperal fever		8	11		—		8	15		1
Deaths										
Puerperal pyrexia‡	159	18	11	1	—	157	9	21	3	—
Deaths		—					—			
Relapsing fever	—	—			—	—	—			—
Deaths										
Scarlet fever	1,768	151	375	39	31	2,176	44	322	48	59
Deaths	—	—	—	—	—	2	—	—	—	—
Smallpox	—	—	—	—	—	—	—	—	—	—
Deaths				—	—				—	—
Typhoid fever	5	—	1	8	1	9	—	6	10	4
Deaths	—	—	—	—	—	—	—	—	—	—
Typhus fever	—	—	—	—	—	—	—	—	—	—
Deaths				—	—				—	—
Whooping-cough*	1,087	88	44	44	2	970	49	78	19	12
Deaths	3	—	—	—	—	6	—	—	1	—
Deaths (0–1 year)	322	31	52	34	12	334	32	72	43	20
Infant mortality rate (per 1,000 live births)										
Deaths (excluding still-births)	4,082	662	535	177	117	4,288	573	605	235	108
Annual death rate (per 1,000 persons living)			12·1	11·4	§			13·9	15·2	§
Live births	6,519	827	713	397	225	6,559	478	997	357	266
Annual rate per 1,000 persons living			14·3	25·6	§			20·3	23·2	§
Stillbirths	202	26	23			202	17	23		
Rate per 1,000 total births (including stillborn)			31					23		

* Measles and whooping-cough are not notifiable in Scotland, and the returns are therefore an approximation only.

† Includes primary form for England and Wales, London (administrative county), and Northern Ireland.

‡ Includes puerperal fever for England and Wales and Eire.

§ Owing to movements of population, birth and death rates for Northern Ireland are still not available.

Letters, Notes, and Answers

All communications with regard to editorial business should be addressed to THE EDITOR, BRITISH MEDICAL JOURNAL, B.M.A. HOUSE, TAVISTOCK SQUARE, LONDON, W.C.1. TELEPHONE: EUSTON 2111. TELEGRAMS: *Aitiology Westcent, London.* ORIGINAL ARTICLES AND LETTERS forwarded for publication are understood to be offered to the *British Medical Journal* alone unless the contrary be stated.

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B.M.A. SCOTTISH OFFICE: 7, Drumsheugh Gardens, Edinburgh.

ANY QUESTIONS?

Bed-sores

Q.—*What principle underlies the use of spirit in the prophylaxis and treatment of bed-sores? What are the correct prophylaxis and treatment of bed-sores in view of the fact that any spirit is detrimental to skin lesions?*

A.—When a part of the body is subject to recurrent or prolonged pressure it is essential to keep the area quite clean. This is done at stated intervals by washing it carefully with soap and water. After washing and drying the part with a smooth towel it is customary to apply spirit for three reasons: first, to complete the drying process; secondly, to harden the skin and make it more resistant to pressure; and, thirdly, with the hope that the alcohol may exert an antiseptic action on any organisms in the vicinity. Some authorities do not believe that its action in any of these respects justifies its use, and prefer to place their reliance in the prevention of bed-sores upon a complete relief of pressure on the threatened area.

The treatment of an actual sore includes relief of pressure, the application of antiseptic and astringent solutions, or the use of adhesive strapping across the affected part. For the various substances applied the questioner should consult a textbook on nursing.

Bite of Culex molestus

Q.—*Is there any antivenene for the bite of Culex molestus? A patient has lost 18 days' work this year as a result of successive bites of this insect. Within two hours of the bite there is a local erythema and induration about 2 in. in diameter, but by 24 hours there is gross oedema of the part, usually the hand and forearm, with lymphangitis and lymphadenitis in upper arm and axilla. Sulphonamides, adrenaline injections, and ephedrine are without effect. The condition subsides in 3 to 5 days, but during this period the man cannot work.*

A.—Unfortunately there is no antivenene for protection against the irritant effects of mosquito-bites. Normally people become gradually immune to the severe after-effects; for example, people going to live in the Tropics are sometimes badly affected at first but gradually become more or less indifferent to the actual bites. The best course to be adopted for the patient in question is to use a repellent while he is exposed to the bites (e.g., dimethyl phthalate). If possible the breeding-site of the mosquito should be traced and dealt with.

"Soma" and "Psyche"

Q.—*Is there any drug known at present (apart from the morphine, cocaine, cannabis, benzedrine, and mescaline compounds) which has the specific property of producing euphoria in the human subject? Such an agent, if free from the undesirable side-effects associated with the above-mentioned compounds, might have important applications in the therapy of some forms of mental disorder. Has any research on this subject been carried out or published in recent years?*

A.—The specific euphoric effect of these drugs is very unreliable. All of them, with the exception of benzedrine, produce a clouding of consciousness which is found by some people agreeable (more frequently so with heroin and cocaine) but by others unpleasant. For the most part they produce euphoria in the same way as does alcohol—i.e., by narcotizing the cortex and removing tensions and inhibitions. The euphoria produced by benzedrine does not depend on this mechanism, but is in most persons very slight. It was first introduced to psychiatry by Guttmann and Peoples for the treatment of depressive states, but has been found of comparatively little value for that purpose; its value in the treatment of narcolepsy, enuresis, certain syndromes found to accompany cerebral dysrhythmia, etc., does not depend on any euphoria produced by it.

A drug, such as Aldous Huxley's "soma"—i.e., almost non-toxic but producing a high degree of euphoria—could have enormous social consequences, but would be unlikely to have more than palliative value in the treatment of mental disorder. The treatment of such conditions must depend in the first place on the correction of a dysfunction or the remedy of a deficiency.

Vitamin B in Dried Yeast

Q.—*Do the spores of the yeast organisms persist as such in tablets of dried yeast, and if so, given suitable conditions, do they grow? Are the amounts of the vitamin B complex fairly constant in the various dried-yeast tablets on the market? In other words, if one prescribes a 5-grain yeast tablet, is one prescribing a given dosage of the B vitamins?*

A.—Yeasts do not normally multiply by spores at all, but by the budding of vegetative cells. Dried yeast preparations may contain sufficient viable cells for multiplication to begin if they are moistened and allowed to stay in the presence of a suitable liquid medium. A few spores may be present, but they are of little importance for purposes of further culture. The extent to which sufficient viable vegetative cells will be present must naturally depend on many conditions, including the degree of desiccation to which the product has been submitted and the length of time that it has remained dry, as well as, probably, temperature of storage. In general, however, it is better not to regard dried yeast as a suitable source of *living* yeast cells, but rather as a subsidiary food reasonably rich in certain essential nutrients.

The amounts of the various members of the vitamin B complex present even in yeast of one strain are likely to vary somewhat, and the variation will be *pro tanto* greater in harvests from mixed strains. Certain manufacturers of proprietary dried yeast "standardize" for vitamin B_1, by assay and suitable blending of various batches. This must inevitably mean that the other members of the complex, of which riboflavin and nicotinic acid are probably the most important in human nutrition, have simply to "take their chance." Dried yeast should in any event not be used as a standardized source of individual vitamins, but rather as a foodstuff rich in certain nutrients the concentration of which, in a reputable brand, is unlikely to fall below a certain fairly high figure.

Diphtheria Prophylaxis

Q.—*Is it safe to give, without a preliminary Schick test, T.A.F. to a child of 5 years previously immunized at 1 year with A.P.T.? In a busy practice the Schick test takes more time than can be afforded.*

A.—It would be perfectly "safe" to give either T.A.F. or a further "boosting" dose of A.P.T. in these circumstances. The latter would be preferable, since it immunizes better, and children of this age rarely react badly to it. The question does not convey the reason for supposing that such an injection might have harmful effects: if it is meant that some form of sensitization might have occurred, the answer is that the amount of protein in the materials is too small for any such effect.

Antispasmodics

Q.—*Trasentin is said to be a good antispasmodic. Would its continued use (say, 1 tablet t.d.s.) be harmful in any way? (I have in mind the type of patient, described by Alvarez, with a super-hypertonic stomach and spasmodic contractions in the colon.) Incidentally, would it be calculated to relax vascular spasms?*

A.—No reports have been seen of harmful effects from the prolonged use of trasentin. Trasentin is a synthetic analogue of atropine, and it is characteristic of this group of alkaloids that they are relatively free from toxic effects. Although trasentin is described as having a papaverine-like action in relaxing the smooth muscle of the intestine, there is no evidence that it has a similar action on the blood vessels. Atropine itself only dilates the blood vessels when given in toxic doses, and one would not expect trasentin to be superior in this respect.

Osgood-Schlatter's Disease

Q.—*A girl aged 13 has had Osgood-Schlatter's disease for about 12 months. She is training to be a ballet dancer, but is handicapped by the pain in both tibial tubercles. She has been told to rest, but this will interfere with her career. Are there any other methods of treatment, and what is the prognosis if she continues to dance?*

A.—Osgood-Schlatter's disease is comparable with adolescent coxa vara. Both are lesions in which various degrees of epiphysial separation occur as the result of repeated violence or continual static stress in association with metaphysial chondritis, the latter in the more extreme forms being due to constitutional and metabolic disturbances. There is some similarity to certain types of stress fracture. In Osgood-Schlatter's disease the element of extension stress must be realized, and immobilization of the knee in almost complete extension for six weeks or so is desirable, followed by rest from dancing or running for a further two months. In a young dancer failure to treat it seriously will almost certainly delay recovery indefinitely. Various other measures have been recommended.

such as drilling, but conservative measures are sufficient. Rest for the knees at the age of 13 may interfere temporarily, but unimportantly, with her career: a persistently painful tibial tubercle may ruin it.

Causes of Deaf-mutism

Q.—*What are the causes of deaf-mutism—other than the accepted theory of the mother contracting rubella while pregnant? Is it possible that large doses of quinine taken in an attempt to produce abortion in early pregnancy would be responsible for this condition?*

A.—There is no evidence that quinine taken during pregnancy is a cause of deaf-mutism. It is in fact unlikely that any external factors operating before birth can be responsible for more than a small proportion of instances. Deaf-mutism may be hereditary or acquired; an estimate of 50% of cases falling into either category may, perhaps, prove to be not very wide of the mark. Any disease which destroys the hearing sufficiently early in life may be the causal factor—for example, scarlet fever, meningitis, or syphilis.

Simple recessive inheritance accounts for at least the very large majority of the hereditary cases. The criteria by which the type of transmission may be recognized are well exemplified: (*a*) The majority of deaf-mutes are the offspring of normal parents. (*b*) Frequently more than one child in the sibship is affected. (*c*) Consanguineous marriages among the parents are commoner than in the general population. (*d*) Affected persons who marry each other have affected children only. This is the usual finding in deaf-mutism. Certain instances in which the offspring were all normal are to be explained either by the condition being an acquired one in one or both parents (as has sometimes been observed), or it may be that more than one recessive gene may give rise to deaf-mutism—which would also lead to apparent exceptions.

Deaf-mutism is sometimes encountered as part of more complex syndromes. For example, its occurrence in association with retinitis pigmentosa serves to distinguish one variety of that condition. This syndrome, like uncomplicated deaf-mutism, also depends upon a recessive gene.

Maturing of Threadworms

Q.—*How long do threadworms take to mature, and what interval must elapse after the appearance of the last worm to consider the cure certain?*

A.—The period of development, from the ingestion of the infective egg to the time of oviposition by the gravid female, is given by different authorities as from fifteen days to two months; probably it rarely exceeds one month. It must be remembered that under suitable conditions the ova may remain alive and infective for periods up to five days, and possibly longer. In such circumstances the patient may become infected from his own fomites, even when not in contact with infected neighbours.

Safety of Butter and Cheese

Q.—*There is much talk at present of the danger of allowing children to drink unboiled milk. Is there any similar danger in allowing them to eat (a) farm butter, (b) National butter, (c) cheese? I believe that the milk used in making farm butter is not generally heat-treated in any way, but I am not aware by what process National butter and cheese are manufactured.*

A.—For general interest the answer to this question may be slightly expanded. At present about 94% of butter distributed in this country is imported, 4% is home-produced in factories, and 2% is home-produced on farms. Practically all imported and factory butter is made from pasteurized or otherwise heat-treated cream, and may therefore be regarded as free from living tubercle bacilli or other pathogenic bacteria. Farm butter is made from raw cream and must be considered potentially dangerous. Most pathogenic organisms, however, including the tubercle bacillus, die out fairly rapidly in butter, owing partly, perhaps, to the amount of acid produced. Though it would be unjustifiable to dogmatize, it is probably fair to say that the danger of contracting disease from farm butter is confined mainly to the fortnight following its manufacture. It may be added that nearly all farm butter is consumed in farming households, and that very little finds its way to the open market.

Of the cheese consumed in this country, 92% is imported, 7% is home-produced in factories, and 1% is home-produced on farms. Probably not less than 60% of imported and factory cheese is made from heat-treated milk; the remainder, including farm cheese, is made from raw milk. Before the war most "hard" cheeses (Cheddar and Cheshire types) were ripened for several months before being offered for sale—a process that led to the death of all pathogenic bacteria; but, as pointed out in the *Journal* (June 12, 1943, p. 730), it became common during the war to distribute imported cheese from the United States and Canada which had had no adequate period of ripening. That unripened cheese may cause disease is illustrated by the occurrence in Canada during the past few years of three or four outbreaks of typhoid fever traced to this source. These outbreaks were caused by cheese that had been consumed within two or three weeks or so of manufacture, and it is doubtful whether the cheese would have proved infective if it had been kept for longer. Between manufacture in America and distribution in this country several weeks elapse, and it is possible, therefore, since pathogenic bacteria are known to die out almost completely within two months in cheese kept at 50° F. or over, that very few, if any, of these organisms are alive by the time the cheese reaches the consumer in this country. There is not enough evidence to assess accurately the risk of disease being carried by cheese made from raw milk, but, considering the relatively small quantities that are eaten at a time, the danger of infection resulting from the consumption of factory-produced cheese must be very small. On the other hand, cheese produced on farms in this country—particularly if it is of the "soft" or cream cheese variety—should be treated with caution, since such cheese is often eaten within a week or so of production. Children had better avoid it altogether.

LETTERS, NOTES, ETC.

Pasteur Institute during the Occupation

Dr. RONALD MACKEITH (chairman, Medical Committee, Scientific Film Association) writes: I read with interest the leading article on Pasteur (Sept. 29, p. 429). Your readers may like to know that the French Office of Information (54, Queen Anne Street, W.1) have just received from France a film made during the occupation, illustrating the work of the Pasteur Institute. *L' Institut Pasteur* contains also some historical matter, and although designed for the general public it will be of real interest to a medical audience. It is a sound film, available 16 or 35 mm., with a French commentary. The running time is 20 minutes.

Sprouting Legumes

J. A. N. writes: Your editorial note on sprouting legumes suggests that it might be of interest to recall the earliest mention of this expedient that I have so far come across. Capt. T. Forrest in his *Voyage from Calcutta to the Mergui Archipelago* (published in 1792) closes the book with "Thoughts on the Best Mode of Preserving Sea Provisions or of Victualling Ships in Warm Climates." In this chapter he mentions "that great antiscorbutic salted limes, lemons or oranges"; he also relates (p. 135) that he sprouted his peas, a trick which he says he learnt from the Malays. I wonder if this is still practised in Malaya. Did Fürst or Chick and Delf know where and how the idea came to be revived?

Books for Polish Doctors

The Polish Medical Association in the British Empire is appealing for gifts of medical and scientific books to reconstruct the libraries in Poland that have been destroyed in the war and to replace books that were confiscated or stolen during the German occupation. In the efforts now being made to train young people for the work of reconstruction, the greatest problem is the lack of books. In the field of medicine teachers badly need textbooks for their students, and books, reference books, and periodicals to bring their own knowledge abreast of the scientific progress made in recent years. Doctors in Great Britain are invited to offer to the Polish profession any medical or scientific books or periodicals they can spare; the language may be English, German, or French. Their help will be greatly appreciated. Books and periodicals may be sent to the Secretary of the British Medical Association at B.M.A. House, Tavistock Square, W.C.1. Correspondence on the subject should be addressed to Dr. B. Jedlewski, 43, Eaton Place, S.W.1.

Corrigenda

Owing to delay in the post the corrected proof of the letter on significance of the erythrocyte sedimentation rate by Drs. D. G. McIntosh and D. M. Keay was received after we had gone to press with the issue dated Oct. 27. On page 584 in the fourth numbered paragraph (*Disregard of Physiological Variations*), line 20, the figures should be 13 of 53, instead of 18 of 55. In the table the reference to Abeles and Pinner should read 86 in the second column, instead of 91.

Dr. K. R. OGILVIE wishes to correct an error in his letter on Ludwig's angina published on Nov. 3. In column 1 of page 623 when describing Case II he wrote "tracheotomy tube" by mistake for "endotracheal tube."

By an error, the name of Prof. J. R. Marrack, one of the signatories of the Memorandum on Training and Qualifications of Dietitians, was omitted from the second sentence of the introductory paragraph of the report published in the *Journal* of Nov. 3 (p. 617). The sentence should have read: "The committee has summarized its findings in the subjoined memorandum, but two of its members (Miss M. Grant and J. R. Marrack) have dissented from paragraph 1 (concerning membership of the British Dietetic Association and registration as medical auxiliaries)."

SUPPLEMENT TO THE

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STATE HOSPITALS?

The report in a Sunday paper that the State is to take over the voluntary hospitals was followed on Monday and Tuesday by varying accounts of what Mr. Aneurin Bevan's plans are. Whatever may be the truth in these reports, one thing is certain. Mr. Bevan has consulted neither the profession's own representative body, nor the voluntary hospitals, nor the local authorities. The principal organizing bodies concerned are, therefore, ignorant of the Minister's intentions. According to one paper, he has put before the Cabinet a proposal that all voluntary hospitals should come under the State, and the Cabinet has agreed in principle. Another paper suggests that the Minister will declare his plans for a National Health Service before Christmas, and that "when the B.M.A. are asked to meet the Minister the discussion will not be on the form of the health service but on the terms of the service.... The B.M.A. will be allowed to put the case as a trade union...." The same paper gives other details. The Government is apparently to nationalize all hospitals, voluntary and municipal, and will do this through 12 Regional Health Boards to cover the whole of the country. Another prophecy is that Mr. Bevan will abolish the sale and purchase of medical practices; but, we are informed through the same source, fair compensation will be paid. Still another suggestion from what appears to be a well-informed political correspondent is that the terms of service will follow the recommendations of the Spens Committee.

A commentator in the *Evening Standard* suggests that Mr. Bevan is likely to have trouble, especially with the local authorities. Mr. Somerville Hastings, M.P., Chairman of the L.C.C. and for ten years chairman of the L.C.C. Hospitals and Medical Services Committee, is reported to have said that it would be a pity if the unification of the hospitals was achieved by leaving out the local authorities.

Until the profession hears from Mr. Bevan himself what his plans are, no useful comment can be made.

HEARD AT HEADQUARTERS

Local Differentiations

From the point of view of the Local Medical War Committee the West of London was one of the most difficult districts that could be found anywhere. Hammersmith, for example, was always shorter of doctors than Kensington. There were the border cases in which the practice was partly in the committee's area and partly across the road in another borough. It was necessary on one occasion to attend before the Medical Personnel Priority Committee to explain why Kensington was a "black" area. It was easily explained. Kensington is one of those areas which attract an unusual number of doctors who are well on towards retiring age, if not beyond it, and are not, and do not want to be, in very active practice. Yet for the necessarily rough-and-ready purposes of medical man-power each of them counted as one. Statistics are quite inadequate as the tool of such bodies as war committees. The numbers as set out on paper are one thing, and translated into terms of men in practice they become quite another. It was soon brought home to these committees that allocations and determinations are not an arithmetical exercise; full account had to be taken of human factors.

The committees were constituted as democratically as possible. The members were elected annually at a meeting to which all doctors in the area were invited. Their work was marked by great fairness. Every doctor who came before them had the fullest hearing, and the decision was reached after the most careful survey of every side of the question. Dr. Pasmore declared that during the whole of his 4½ years' secretaryship he could not recall one disagreeable episode. Beyond its main task, the committee, especially in the earlier days, was constantly being called upon for the nomination of local doctors for this or that special service—for example, in connexion with the Home Guard, the Medical Boards, and the civil practitioners employed part-time by the military authorities.

Perhaps the work of the local committees is now virtually done, though there will no doubt be reluctance to let an excellent piece of organization disappear. They are being given fresh jobs; the latest is the nomination of medical members of the advisory committees and panels set up under the Disabled Persons (Employment) Act. Some may think they could perform a useful function in the resettlement of men in civil practice. But whether they come to an end or not, they can look back upon an excellent piece of work for the profession and the country.

Public Vaccinators

Some important recommendations on fees for public vaccinators will come before the next meeting of the B.M.A. Council. Following representations by the Association of Public Vaccinators, a representative of which attended at its last meeting, the Public Health Committee of the B.M.A. is asking the Council to express the view that the fees paid to public vaccinators are still wholly inadequate. A more acceptable scale has been formulated in the West Riding of Yorkshire, and has been approved by the Ministry, and in the committee's view the scale should not be less than the West Riding figures; also such fees should be payable whether the vaccination is successful or unsuccessful, and a proper mileage allowance should be paid.

A recommendation will also come before the Council that the necessary modifications be made in the Vaccination Order to allow the vaccination of children under 1 year of age to be carried out at the doctor's surgery where this is thought desirable and expedient, and that a minimum fee of five shillings should be paid for each successful vaccination so performed.

Local Authority Health Functions

Some time ago the L.C.C. put forward its view to the Government that the Council should be constituted as the statutory authority for administering the proposed comprehensive medical service within its area, with the metropolitan borough councils as the local authorities for administering certain parts of the service in their respective boroughs. Last week a report was brought before the council on the delegation of medical duties from the county to the borough councils. These delegated duties are of three kinds: maternity and child welfare, tuberculosis dispensaries, and epidemiological inquiries, this last a linkage between the laboratory service of the boroughs and the fever hospitals and laboratories of the L.C.C.

The delegation of maternity and child welfare from the county to the borough councils is to be exclusive of the institutional work now carried on by the borough councils, which will in the new circumstances be carried on by the county council. This has necessitated a definition of "institutional work"—namely, the provision, directly or indirectly, by a maternity or child welfare authority of residential accommodation for expectant or nursing mothers and their children who have not attained the age of 5 years and are not being educated in schools recognized by the Ministry of Education.

Tuberculosis dispensaries, the L.C.C. lays it down, should be situated in or near a general hospital at which a consultant service of all types and facilities for *x*-ray examinations, etc., are available, but they should retain their separate identity. One

such as drilling, but conservative measures are sufficient. Rest for the knees at the age of 13 may interfere temporarily, but unimportantly, with her career: a persistently painful tibial tubercle may ruin it.

Causes of Deaf-mutism

Q.—*What are the causes of deaf-mutism—other than the accepted theory of the mother contracting rubella while pregnant? Is it possible that large doses of quinine taken in an attempt to produce abortion in early pregnancy would be responsible for this condition?*

A.—There is no evidence that quinine taken during pregnancy is a cause of deaf-mutism. It is in fact unlikely that any external factors operating before birth can be responsible for more than a small proportion of instances. Deaf-mutism may be hereditary or acquired; an estimate of 50% of cases falling into either category may, perhaps, prove to be not very wide of the mark. Any disease which destroys the hearing sufficiently early in life may be the causal factor—for example, scarlet fever, meningitis, or syphilis.

Simple recessive inheritance accounts for at least the very large majority of the hereditary cases. The criteria by which the type of transmission may be recognized are well exemplified: (*a*) The majority of deaf-mutes are the offspring of normal parents. (*b*) Frequently more than one child in the sibship is affected. (*c*) Consanguineous marriages among the parents are commoner than in the general population. (*d*) Affected persons who marry each other have affected children only. This is the usual finding in deaf-mutism. Certain instances in which the offspring were all normal are to be explained either by the condition being an acquired one in one or both parents (as has sometimes been observed), or it may be that more than one recessive gene may give rise to deaf-mutism—which would also lead to apparent exceptions.

Deaf-mutism is sometimes encountered as part of more complex syndromes. For example, its occurrence in association with retinitis pigmentosa serves to distinguish one variety of that condition. This syndrome, like uncomplicated deaf-mutism, also depends upon a recessive gene.

Maturing of Threadworms

Q.—*How long do threadworms take to mature, and what interval must elapse after the appearance of the last worm to consider the cure certain?*

A.—The period of development, from the ingestion of the infective egg to the time of oviposition by the gravid female, is given by different authorities as from fifteen days to two months; probably it rarely exceeds one month. It must be remembered that under suitable conditions the ova may remain alive and infective for periods up to five days, and possibly longer. In such circumstances the patient may become infected from his own fomites, even when not in contact with infected neighbours.

Safety of Butter and Cheese

Q.—*There is much talk at present of the danger of allowing children to drink unboiled milk. Is there any similar danger in allowing them to eat (a) farm butter, (b) National butter, (c) cheese? I believe that the milk used in making farm butter is not generally heat-treated in any way, but I am not aware by what process National butter and cheese are manufactured.*

A.—For general interest the answer to this question may be slightly expanded. At present about 94% of butter distributed in this country is imported, 4% is home-produced in factories, and 2% is home-produced on farms. Practically all imported and factory butter is made from pasteurized or otherwise heat-treated cream, and may therefore be regarded as free from living tubercle bacilli or other pathogenic bacteria. Farm butter is made from raw cream and must be considered potentially dangerous. Most pathogenic organisms, however, including the tubercle bacillus, die out fairly rapidly in butter, owing partly, perhaps, to the amount of acid produced. Though it would be unjustifiable to dogmatize, it is probably fair to say that the danger of contracting disease from farm butter is confined mainly to the fortnight following its manufacture. It may be added that nearly all farm butter is consumed in farming households, and that very little finds its way to the open market.

Of the cheese consumed in this country, 92% is imported, 7% is home-produced in factories, and 1% is home-produced on farms. Probably not less than 60% of imported and factory cheese is made from heat-treated milk; the remainder, including farm cheese, is made from raw milk. Before the war most "hard" cheeses (Cheddar and Cheshire types) were ripened for several months before being offered for sale—a process that led to the death of all pathogenic bacteria; but, as pointed out in the *Journal* (June 12, 1943, p. 730), it became common during the war to distribute imported cheese from the United States and Canada which had had no adequate period of ripening. That unripened cheese may cause disease is illustrated by the occurrence in Canada during the past few years of three or four outbreaks of typhoid fever traced to this source. These outbreaks were caused by cheese that had been consumed within two or three weeks or so of manufacture, and it is doubtful whether the cheese would have proved infective if it had been kept for longer. Between manufacture in America and distribution in this country several weeks elapse, and it is possible, therefore, since pathogenic bacteria are known to die out almost completely within two months in cheese kept at 50° F. or over, that very few, if any, of these organisms are alive by the time the cheese reaches the consumer in this country. There is not enough evidence to assess accurately the risk of disease being carried by cheese made from raw milk, but, considering the relatively small quantities that are eaten at a time, the danger of infection resulting from the consumption of factory-produced cheese must be very small. On the other hand, cheese produced on farms in this country—particularly if it is of the "soft" or cream cheese variety—should be treated with caution, since such cheese is often eaten within a week or so of production. Children had better avoid it altogether.

LETTERS, NOTES, ETC.

Pasteur Institute during the Occupation

Dr. RONALD MACKEITH (chairman, Medical Committee, Scientific Film Association) writes: I read with interest the leading article on Pasteur (Sept. 29, p. 429). Your readers may like to know that the French Office of Information (54, Queen Anne Street, W.1) have just received from France a film made during the occupation, illustrating the work of the Pasteur Institute. *L' Institut Pasteur* contains also some historical matter, and although designed for the general public it will be of real interest to a medical audience. It is a sound film, available 16 or 35 mm., with a French commentary. The running time is 20 minutes.

Sprouting Legumes

J. A. N. writes: Your editorial note on sprouting legumes suggests that it might be of interest to recall the earliest mention of this expedient that I have so far come across. Capt. T. Forrest in his *Voyage from Calcutta to the Mergui Archipelago* (published in 1792) closes the book with "Thoughts on the Best Mode of Preserving Sea Provisions or of Victualling Ships in Warm Climates." In this chapter he mentions "that great antiscorbutic salted limes, lemons or oranges"; he also relates (p. 135) that he sprouted his peas, a trick which he says he learnt from the Malays. I wonder if this is still practised in Malaya. Did Fürst or Chick and Delf know where and how the idea came to be revived?

Books for Polish Doctors

The Polish Medical Association in the British Empire is appealing for gifts of medical and scientific books to reconstruct the libraries in Poland that have been destroyed in the war and to replace books that were confiscated or stolen during the German occupation. In the efforts now being made to train young people for the work of reconstruction, the greatest problem is the lack of books. In the field of medicine teachers badly need textbooks for their students, and books, reference books, and periodicals to bring their own knowledge abreast of the scientific progress made in recent years. Doctors in Great Britain are invited to offer to the Polish profession any medical or scientific books or periodicals they can spare; the language may be English, German, or French. Their help will be greatly appreciated. Books and periodicals may be sent to the Secretary of the British Medical Association at B.M.A. House, Tavistock Square, W.C.1. Correspondence on the subject should be addressed to Dr. B. Jedlewski, 43, Eaton Place, S.W.1.

Corrigenda

Owing to delay in the post the corrected proof of the letter on significance of the erythrocyte sedimentation rate by Drs. D. G. McIntosh and D. M. Keay was received after we had gone to press with the issue dated Oct. 27. On page 584 in the fourth numbered paragraph (*Disregard of Physiological Variations*), line 20, the figures should be 13 of 53, instead of 18 of 55. In the table the reference to Abeles and Pinner should read 86 in the second column, instead of 91.

Dr. K. R. OGILVIE wishes to correct an error in his letter on Ludwig's angina published on Nov. 3. In column 1 of page 623 when describing Case II he wrote "tracheotomy tube" by mistake for "endotracheal tube."

By an error, the name of Prof. J. R. Marrack, one of the signatories of the Memorandum on Training and Qualifications of Dietitians, was omitted from the second sentence of the introductory paragraph of the report published in the *Journal* of Nov. 3 (p. 617). The sentence should have read: "The committee has summarized its findings in the subjoined memorandum, but two of its members (Miss M. Grant and J. R. Marrack) have dissented from paragraph 1 (concerning membership of the British Dietetic Association and registration as medical auxiliaries)."